THE ACCOUNTING REVIEW

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VOLUME 81, No. 1 JANUARY 2006 4/10/07

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The Accounting Review (ISSN 0001-4826 print and ISSN 1558-7967 online) is published five times a year in January, March, May, July, and October by the American Accounting Association, 5717 Bessie Drive, Sarasota, Florida 34233-2399. General subscriptions are available at the annual rate of U.S. $300; membership subscription rate is $25 with discounts available; and emeritus subscription rate is $8.75. Periodical postage paid at Sarasota, Florida and additional mailing offices. **POSTMASTER:** Send address change to *The Accounting Review,* 5717 Bessie Drive, Sarasota, Florida 34233-2399.

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pp. 1–27

CEOs'/CFOs' Swearing by the Numbers: Does It Impact Share Price of the Firm?

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ABSTRACT: We examine the impact on share prices of firms whose CEOs and CFOs certify their financial statements under oath, pursuant to the administrative order issued by the SEC on June 27, 2002. We hypothesize that (1) the certification provides assurance to investors by making disclosure more credible and by reducing information asymmetry between owners and management, and (2) the assurance value of certification is reflected in the stock price of the certifying company. Overall, the empirical results are consistent with our hypotheses. We observe, on average, positive abnormal returns for firms whose CEOs/CFOs certified their financial statements by August 14, 2002. Based on an analysis of bid-ask spreads, certifying firms experienced a significant decline in information asymmetry after certification. In cross-sectional analyses, we find abnormal returns are positively associated with firms that were under investigation, that used Andersen as their auditor, and that practiced aggressive revenue recognition.

Keywords: *SEC; CEO; CFO; assurance; information asymmetry; certification; credible disclosure; share price.*

We gratefully acknowledge the comments and suggestions of Rajiv Banker, Chang-Jane Lin, Mei-Hwa Lin, Suming Lin, Chih-Chun Liu, Raj Mashruwala, Wayne Thomas, Niranjan Tripathy, Mark Vargus, Kam-Ming Wan, Rebecca Wu, and participants of the accounting workshops at: the University of California, Riverside; the University of California, Irvine; The University of Texas at Dallas; National Changhua University of Education; National Chung Hsing University; National Taipei University; and National Taiwan University. We thank Hsu-Wei Hsu, Monica Johar, Yia-Hsiang Kang, Tiffany Yang, and the webmaster at the SEC for their help in this research. We owe a special debt of gratitude to Professor Richard Painter at the University of Illinois at Urbana–Champaign Law School for his expert interpretations. We are grateful to two anonymous reviewers for their suggestions. Names of authors are listed alphabetically, independent of their contribution.

Editor's note: This paper was accepted by Lawrence Brown, Editor.

Submitted October 2002
Accepted August 2005

Data Availability: *Data used in this study are available from public sources identified in the study.*

I. INTRODUCTION

The collapse of the Internet bubble and the severe downturn of the stock market claimed a series of high-profile corporate failures from both the new and old economies at the dawn of the 21st century. One-time corporate giants such as Enron, Adelphia, Global Crossing, WorldCom, Kmart, and US Airways followed one another into Chapter 11 bankruptcy protection. By the beginning of August 2002, 12 corporations including AOL, Xerox, Merck, Bristol-Myers Squibb, and Citigroup were under investigation, and more than ten senior executives were arrested in connection with financial misdeeds. These business failures were plagued by a series of accounting irregularities and outright fraudulent business reporting practices that caused investors to panic and undermined their trust in corporate America. Revelations of accounting irregularities—and the high likelihood that additional irregularities would be uncovered—led to a severe decline of U.S. capital market indices, which, in turn took a toll on investors' psyches.[1]

Both regulators and lawmakers realized that the loss of public confidence in corporate America could be devastating for the U.S. economy because more than half of all U.S. households had invested, either directly or through their pension plans, in U.S. capital markets. In July 2002, while announcing tougher penalties for corporate crimes, President Bush stated, "A key to our economic development is consumer and investor confidence in the markets and in the integrity of corporate America, and right now that confidence has been shaken" (Gongloff 2002). During the same time period, then SEC Chairman Harvey Pitt said, "The seemingly endless drumbeat of Enron, Global Crossing, Tyco, ImClone ... has caused investors around the globe to lose confidence in American business and to question the basic integrity. If that is allowed to persist, a key pillar on which our capital markets is premised will crumble" (Pitt 2002). Both the economic and political primacy of the issue led to a raft of legislative and administrative measures to improve financial reporting and corporate governance.

One high-profile measure was the SEC requirement that CEOs and CFOs certify their financial results under oath. On June 27, 2002, the SEC issued an administrative order requiring the CEOs and CFOs of U.S. corporations with revenue of at least $1.2 billion in their latest fiscal year (947 firms) to provide sworn testimony of their financial results. The majority (688) of these corporations were required to certify under oath the veracity of their financial statements by August 14, 2002.

Using event-study methodology, we report the impact of such certifications on firms' market values. We find positive abnormal returns for firms whose CEOs/CFOs certified their financial statements by August 14, 2002, and we show that bid-ask spreads declined for these firms in the post-certification period. Using regression analysis, we find that market model-based abnormal returns are positively associated with firms under investigation by the SEC and the Department of Justice (hereafter, under investigation), with firms using Andersen as their auditor, and with firms reporting higher revenue growth. We also find that abnormal returns for noncomplying firms (All Others) are significantly lower than those of a peer group of firms in the same industry.

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[1] A monthly survey of "investor optimism" conducted by UBS and Gallup found the mood among investors on June 24, 2002 to be almost as grim as it was after September 11—sinking by nearly half since the giddy days of late 1999 and early 2000. Similarly, the average daily trading volume at Charles Schwab & Co.—another barometer of investor confidence—was down 54 percent from the height of the bull market (Nocera 2002).

Our results provide evidence that the SEC order requiring filing of sworn statements by CEOs and CFOs had a positive effect on the market value of certifying firms. This effect is consistent with the notion that certification improved investors' confidence in corporate disclosures. Given our findings, it can be inferred that enacting the sworn certification of financial statements as a permanent reporting requirement under the Sarbanes-Oxley Act of 2002 likely resulted in improved investors' confidence in corporate reporting.

The remainder of the paper is organized as follows: Section II describes the background of the SEC order and its implications for the market value of certifying companies and then develops the research hypotheses tested in this study. Section III discusses research design including sample selection, variable measurement, and estimation model. Section IV presents and discusses empirical results. Section V concludes with a brief summary of our results.

II. BACKGROUND OF THE SEC ORDER, SUBSEQUENT LEGISLATION, AND ITS IMPLICATIONS FOR FIRMS' SHARE PRICES

Background of the SEC Order and Subsequent Legislation

New Exchange Act Rule 13a-14

In a bid to turn around investor confidence, the SEC proposed the New Exchange Act Rule 13a-14 on June 12, 2002, which applied to all domestic companies subject to the reporting requirements of the Securities Exchange Act of 1934. This rule required both the CEO and the CFO to certify quarterly and annual financial reports to the following facts:

- He or she has read the report;
- To his or her knowledge, the information in the report is true in all important respects as of the last day of the period covered by the report; and
- The report contains all information about the company of which he or she is aware that he or she believes is important to a reasonable investor as of the last day of the period covered by the report.

For certification purposes, Rule 13a-14 defines information to be important to a reasonable investor if: (1) there is a substantial likelihood that the information would significantly alter the investor's beliefs and/or (2) the report would be misleading if the information was omitted from the report. In addition, Rule 13a-14 requires a company to maintain records and procedures to provide reasonable assurance that it is able to collect, process, and disclose the information required in its periodic and current reports pursuant to the Exchange Act. The new rule also requires an annual review and evaluation of these procedures by the company that would be presented to the company's CEO and CFO. Moreover, the CEOs and CFOs are required to certify in the company's annual report that they have reviewed the results of the evaluations.[2]

The SEC Order and its Relation to the Sarbanes-Oxley Act of 2002

On June 27, 2002, the SEC issued an order requiring the CEOs and CFOs of U.S. publicly traded companies with revenues of at least $1.2 billion (947 firms) in their latest fiscal years to file with the SEC sworn statements regarding the accuracy and completeness of their companies' most recently filed Form 10-K and other subsequently filed periodic reports and definitive proxy statements. A list of these companies is posted on the SEC website: http://www.sec.gov.

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[2] Details are available at SEC (2002a).

The order requires CEOs and CFOs of the subject companies to file separate statements under oath that, to the best of their knowledge, each of the company's "covered reports" is true in all material respects and does not contain any material omissions. These reports include the company's most recent annual report on Form 10-K, all quarterly reports on Form 10-Q, current reports on Form 8-K, proxy materials filed after the filing of the Form 10-K, and any amendments to the foregoing. Failing to make such a statement, the officer must submit a sworn statement describing the facts and circumstances that would make such a statement incorrect. In addition, the sworn statement must indicate whether the officer has reviewed the statement with the company's audit committee or, in the absence of an audit committee, with the company's independent directors. The relevant date for the accuracy and completeness of a covered report is the end of the period covered by such report (or in the case of a report on Form 8-K or definitive proxy materials, as of the date on which they were filed)[3] except when corrected or supplemented in a subsequent covered report.

Although the SEC order was supposed to be a one-time requirement, subsequent to the order a new act (the Sarbanes-Oxley Act of 2002) was passed by the U.S. Congress and signed by President Bush on July 30, 2002. This act requires certification of financial statements by the CEO and the CFO of the issuing company. Under this act, those who knowingly file a statement that does not conform to legal requirements face up to ten years in prison and $1 million in fines, while for a "willfully" false certification, the maximum penalty is 20 years in prison and $5 million in fines. Thus, this act, in effect, perpetuates the SEC's order for one-time certification that was issued on June 27, 2002.

Implications of the SEC Order for the Certifying Firm

Reaction in the popular press about the ability of the SEC order of June 27, 2002, to restore investor confidence in corporate America was mixed. However, anecdotal evidence and logical reasoning suggested that although investors were skeptical, the SEC certification could help investors regain some of their confidence in the capital markets.[4] While ordering the sworn statements from CEOs and CFOs, the SEC noted the following:

> In light of recent reports of accounting irregularities at public companies, including some large and seemingly well-regarded companies, the purpose of the Commission's investigation is to provide *greater assurance* to the Commission and to investors that persons have not violated, or are not currently violating, the provisions of the federal securities laws governing corporate issuers' financial reporting and accounting practices, and to aid the Commission in assessing whether it is necessary or appropriate in the public interest or for the protection of investors for the Commission to adopt or amend rules and regulations governing corporate issuers' reporting and accounting practices and/or for the Commission to recommend legislation to Congress concerning these matters. (SEC 2002b) (emphasis added)

Thus, one of the major goals of the SEC in ordering the certification was to assure investors through credible disclosures that, while there was misreporting in the past, most companies had sound financial reporting and accounting practices. Moreover, the sworn statements from CEOs and CFOs would ensure that these executives could no longer feign lack of knowledge as did the top executives of Enron and WorldCom. Prior to promulgation of the SEC order, with respect to proving criminal misconduct, prosecutors required proof that an executive willfully broke the law—which he or she knew—by stating, for example, that a fraudulent financial statement was true. However, if the executive had any legitimate reason for believing that the financial statements were accurate, criminal charges would fail under the legal standard of reasonable doubt. This legal standard, along with the fact that most executive decisions are backed up by paper trails that may include advice from accountants, outside attorneys, board approval, files, and memos justifying the decision, made it easy for executives to deflect blame away from themselves. However, most legal experts believe that the liability situation changed dramatically under the SEC order.[5]

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[3] Please see SEC (2002b) for more details.

[4] "It was a frenzy that got out of hand," said Weeden strategist Steve Goldman. "The certifications calmed things down." Similarly, Banc One Capital Markets' head of investment grade research, Dave Novosel, said "If you had asked me on August 13, I would have said certification didn't matter. But it definitely was a factor. It had more significance than we originally thought" (Lahart 2002).

In general, filing a false statement with any governmental agency is a criminal offense. The SEC order *expanded* (i.e., in addition to earlier exposures) an executive's potential exposure to criminal prosecution by subjecting the executive to potential charges of perjury (false statement under oath), obstruction of justice (impeding any investigation pursuant to the order or otherwise), and mail or wire fraud. A principal executive officer or principal financial officer providing a false certification potentially could be subject to SEC enforcement action for violating sections 10(b) and 13(a) of the Exchange Act and could be liable in civil actions by stockholders for violating Section 10(b) of the Exchange Act and Exchange Act Rule 10b5. In addition, an officer's verification that he or she was personally involved in reviewing and approving the periodic filings may diminish the defenses available to the officer for claims made under the Exchange Act Section 10(b), including Rule 10b5 promulgated there under. A CEO or CFO who personally attests to having reviewed the subject statements with the audit committee will have a less credible argument that he or she did not directly or indirectly induce the acts constituting the alleged violation.[6] In fact, compared to Enron and WorldCom, the success of the SEC in prosecuting several high-ranking officials of HealthSouth Corporation on criminal and fraud charges has been ascribed to the certification requirement under the Sarbanes-Oxley Act of 2002 (McClam 2003).

The certification by CEOs/CFOs had several implications for corporate disclosures. Many CEOs demanded that each reporting segment of their business or line of business certify that its numbers meet the certification standard or that it identifies the transactions that do not meet the SEC standards.[7] This CEO demand has the cascading effect that each officer in the reporting hierarchy explicitly or implicitly requires an assurance from the next lower level that the reported numbers are correct. Thus, when certifying their financial statements, the incentive of CEOs and CFOs is to protect themselves from criminal and civil liabilities.[8]

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[5] We undertook a thorough web search of law and investment firms' opinions of CEOs'/CFOs' certification. Several law firms practicing corporate and securities law issued alerts and recommendations on the web. Most of them believed that certification increased potential legal liabilities for the CEOs/CFOs. We did not find a single law firm stating that certification was irrelevant.

[6] According to Professor Henry Hu at The University of Texas at Austin Law School and Professor Richard Painter at the University of Illinois at Urbana–Champaign Law School, certification increased potential liabilities under civil and criminal prosecution, including PSLRA. (Both Professors Hu and Painter are experts in corporate and securities law and market regulation. Professor Painter authored sections of the Sarbanes-Oxley Act.).

[7] Mr. Byron Pollitte, Jr., the CFO of GAP, Inc., mentioned this in a conversation with the authors. He further added that most of his subordinates believe that if the numbers are false they may end up going to jail.

[8] For example, when certifying the results of AOL, the CEO and the CFO flagged $49 million of revenue they believed might have been mistakenly recognized by the online division. Similarly, the CEOs and CFOs of Bristol-Myers Squibb and Interpublic Group also certified their results with caveats (CNNmoney 2002).

These certifications were expected to reduce investors' uncertainties that a future Enron or WorldCom might be brewing and to restore their confidence in the reported financial statements by making disclosures more credible. In addition, because a vast majority of firms did not have problems in certifying their statements, firms would send a signal to shareholders that transparent financial reporting and good corporate governance are the rule rather than the exception in corporate America.

The above arguments follow the tradition of the economics of crime literature, which views crime as a rational decision made by potential offenders (Becker 1968; Rasmussen 1995; Sethi 1979; Shavell 1991; Stigler 1970). The main argument of this literature is that increasing the potential penalty discourages the marginal potential offender from committing crime. Based on this literature's conclusions, the SEC certification increases potential civil and criminal liabilities and thus deters executives from misreporting financial statement numbers.

While the economics of crime literature suggests that certification would decrease misreporting and increase investors' confidence, there were skeptics among the general investing public along with experts. One argument was that the administrative order by the SEC was invalid (i.e., that the SEC is not a rule-making body) and, hence, the SEC could not penalize non-certifiers.[9] This view was supported by legal scholars as technically valid.[10] Others argued that the certification requirement is unusually harsh.[11] However, investors were likely to penalize a noncompliant firm, even if the SEC was unable to do so under the administrative order. Moreover, as an enforcement agency, the SEC can conduct an independent investigation of the noncomplying company's accounting practices.

Rational investors are believed to revise their expectations based on new information in announcements, conditioned on all publicly available information to date. Aggregated investors' expectations are reflected in stock prices. The certification by CEOs and CFOs reduced information asymmetry between management and owners about a firm's financial condition. In other words, even if the top executives know that their firm's financial statement numbers accurately represent its current condition and future prospects, investors may not believe the numbers completely before the certification, as they are likely to discount them in light of the financial misreporting by other firms. This causes a high degree of uncertainty in investors' minds about the information reported in financial statements and leads to a high degree of information asymmetry.[12]

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[9] J. Edward Ketz of Penn State's Smeal College of Business Administration writes, "To begin, the certifications were *ad hoc* and possibly with no legal substance. No law exists that required the certifications, as can be seen when SEC officials admit that they have no idea what would happen to those managers who do not file certifications. With no consequences, there will be no change in behavior" (Ketz 2002).

[10] Henry Hu of The University of Texas at Austin Law School, who strongly believes that the potential legal liability has increased under certification, agrees that Ketz's argument above is technically valid (Hu 2002).

[11] Don Luskin, the Chief Investment Officer for Trend Macrolytics, in an opinion piece in *Capitalism* magazine, called the law requiring CEO certification of financial statements "the work of idiots." However, unlike Ketz (2002) he argued that the certification actually imposes tremendous liability on the CEOs. He wrote, "When bad laws make criminals of innocent CEOs, innocent men won't want to be CEOs any more—only corrupt men will" (Luskin 2004).

Recent research (Botosan 1997; Easley and O'Hara 2004; Easley et al. 2002, 2004; Francis et al. 2003a, 2003b) documents that quantity and quality of information affects cost of capital and asset prices. Botosan (1997) observes that greater disclosure is associated with lower cost of equity capital. Based on a measure of voluntary disclosure, she finds that a one-unit difference in her disclosure measure is associated with a difference of approximately 28 basis points in the cost of capital. Using a multi-asset rational expectation model, Easley and O'Hara (2004) show that investors demand a higher return to hold stocks with greater private information. The model illustrates how in equilibrium the quantity and quality of information affects asset prices. Easley et al. (2002) show that information does affect asset pricing fundamentally, and a difference of 10 percentage points in the probability of information-based trading between two stocks leads to a difference of 2.5 percent expected return on an annual basis. Easley et al. (2004) extend their earlier research to examine the potential profit of trading on this information risk. Francis et al. (2003a) show that lower accrual quality increases the cost of capital. Francis et al. (2003b) show that information uncertainties as proxied by earnings quality can partially explain accounting-based trading anomalies, such as post-earnings announcement drift, value-glamour, and accrual strategies.

Certification makes investors more likely to believe that the numbers disclosed by firms are valid. By providing assurance and decreasing information asymmetry between management and owners about the current state and future prospects of the firm, the SEC order is likely to improve investors' confidence in corporate disclosures, thereby reducing information risk and the cost of capital. If investors have already heavily discounted possible bad news before certifications, as shown in the survey of investor optimism by UBS and Gallup Group and other anecdotal evidence, then certification by CEOs and CFOs should raise the firm's value.[13] Thus, the capital markets would respond favorably to the certification by CEOs and CFOs.

Based on the above arguments, we expect a positive stock market reaction for certifying firms around the event window and pre-certification information asymmetry to be larger than post-certification information asymmetry. Accordingly, our first two hypotheses are stated below. (All hypotheses are stated in alternate form.)

H1: The stock market reaction during the event window related to the certification of the firm's financial reports by the CEO and the CFO is positive.

H2: The bid-ask spread decreases in the post-certification period.

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[12] A stream of research in accounting has examined how accounting information affects information asymmetry in the stock market. Lev (1988) argues that better disclosure reduces information asymmetry, leading to smaller bid-ask spreads and lower transaction costs in stock markets. Greenstein and Sami (1994) examine the impact of the SEC's segment disclosure requirement on bid-ask spreads and find that segment disclosure reduces information asymmetry. Coller and Yohn (1997) investigate whether the decision to issue a management forecast of earnings is related to information asymmetry and find that forecasting firms, relative to a matched sample of non-forecasting firms, have larger bid-ask spreads prior to the forecast release that disappear when the forecast is released. Affleck-Graves et al. (2002) find that firms with more predictable earnings have less information asymmetry compared to firms with relatively less predictable earnings. See Callahan et al. (1997) for a comprehensive survey of academic articles dealing with accounting information and information asymmetry.

[13] Certification does not affect the expected cash flow unless it influences implicit claims with stakeholders. However, certification can reduce the cost of capital and thus increase the present value of the firm.

Factors Affecting Cross-Sectional Variation in Abnormal Stock Returns

Aggressive Revenue Recognition Practice

Aggressive revenue recognition was one of the areas of which the Power Committee was highly critical of Enron's financial reporting (Powers et al. 2002). The evidence of aggressive or fraudulent revenue recognition practices led the SEC to investigate more than ten companies, including Enron, WorldCom, Qwest, Merck, and AOL. The importance of aggressive revenue recognition started during the stock market bubble of the late 1990s, when investors valued highly the firms that could grow their revenues quickly. In fact, most of the so-called new economy firms had little, if any, positive earnings and were valued based on their revenue increases and other criteria such as page views and number of unique visitors (Davis 2002). Both Enron and WorldCom had high year-to-year revenue growth and were handsomely rewarded by the stock market.[14]

Share valuation based on revenue growth led several firms to use "creative" or fraudulent methods to keep showing revenue increases year after year. For example, Dynergy settled charges of inflating revenue by $300 million by paying a penalty of $3 million to the SEC (Houston Business Journal 2002). Erickson et al. (2004) find that the probability of accounting fraud increases in the percentage of total executive compensation that is stock-based after controlling for governance characteristics, financial performance, financial distress, firm size, and likelihood of management trying to get external financing. In their sample, a one standard deviation increase in the proportion of compensation that is stock-based increases the probability of accounting fraud by 68 percent. Bartov and Mohanram (2004) investigate the decision by corporate executives of more than 1,200 public corporations to exercise large stock option awards during the period 1992–2001. They find that the disappointing earnings in the post-exercise period represent a reversal of inflated earnings in the pre-exercise period.

Thus, investors had a sound reason to be skeptical about the quality of revenue growth and its implications for the firm's value. Under such circumstances, investors would discount high revenue increases when valuing firms, as it was unclear whether the increased revenue resulted from underlying business growth or aggressive revenue recognition. Consistent with these arguments, our third hypothesis is:

H3: The abnormal stock market return due to certification of the firm's financial reports by the CEO and the CFO is positively associated with aggressive revenue recognition.

SEC Investigation

Several firms on the SEC list were under investigation for misleading financial reporting practices, including AOL, Merck, and Xerox. We list these firms and the reason for their investigation in the Appendix. Most investigations were ongoing at the time of the certification. Investors were highly uncertain about the financial reports and the reporting practices of these companies, so certification of financial reports by CEOs and CFOs of these corporations should be good news for investors, by providing a higher degree of assurance. Accordingly, we hypothesize the following:

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[14] Enron reported sales growth of 151 percent between 1999 and 2000 and a 28 percent increase from 1998 to 1999. WorldCom reported sales growth of 110 percent during 1998–1999. In addition, Power's report about Enron also notes that aggressive revenue recognition was one critical area in Enron's reporting.

H4: The abnormal stock market return due to the certification of the firm's financial reports by the CEO and the CFO is higher for firms under investigation.

Andersen's Clients

The extent of Andersen's involvement in Enron's accounting irregularities created a unique situation in the history of the assurance industry. With a series of improper audit practices beginning in 1999 with Sunbeam and continuing with the Baptist Foundation of Arizona, Waste Management, and Enron, Andersen witnessed a dramatic decline in reputation and eventual bankruptcy.[15] Prior studies have argued that the role of the audit is to provide monitoring to reduce the information asymmetry between owners and managers, and to provide insurance to cover investors' losses due to the deep pocket effect (Kothari et al. 1988; Schipper 1991). Menon and Williams (1994) and Baber et al. (1995) provided evidence of an insurance effect by documenting negative client security price reactions to the Laventhol and Horwath bankruptcy case. Most recently, Asthana et al. (2002) and Chaney and Philipich (2002) examined Andersen's other clients' stock price reactions surrounding event windows, when Andersen's audit procedures and independence were under severe scrutiny, and found abnormal negative returns surrounding those windows. Because Andersen is bankrupt, it cannot provide insurance to its clients. Normally, however, such insurance would be reflected in the stock prices of an auditor's clients, due to the likelihood that the insurance right would be exercised.[16] For example, if investors believe there is no problem with a client's audit, then this insurance has no value for that particular client. Thus, the value of the insurance is positively related to the probability of the firm's audit failure. Given the nature and scale of Andersen's involvement in Enron and other companies that had Andersen as their auditor, investors were uncertain about the financial numbers reported by Andersen's clients.[17] In this scenario, certification of the financial reports by CEOs and CFOs under oath gave credibility to the financial reports by these client companies, providing both assurance and undoing some of the negative effect on their stock prices due to Andersen's bankruptcy and concomitant loss of insurance. Investors should consider this outcome as good news, because it reduced uncertainties due to audit failure related to Andersen's involvement. Hence, our fifth hypothesis is:

H5: The abnormal stock market return due to the certification of the firm's financial reports by the CEO and the CFO is higher for firms that were Andersen's clients.

III. RESEARCH DESIGN

This section describes our sample selection procedure, explains how we measure the market reaction to the filing of CEOs'/CFOs' sworn statements, and specifies a regression model for testing our hypotheses.

Sample Selection

We obtained an initial sample of 688 firms required by the SEC to file CEOs'/CFOs' sworn statements by August 14, 2002. We kept firms that met the criteria: (1) stock trading price data available on the Yahoo! Finance website, (2) financial data reported in the 2001 annual Compustat dataset, (3) stock trading prices of at least \$0.25 per share, and (4) major confounding events that did not overlap with the five-day window (−2, +2) surrounding the receipt date of sworn statements.[18] Criteria (1) and (2) are necessary to meet the complete data requirement; criterion (3) reduces the effect of outlier returns generated from low value shares; and criterion (4) removes the confounding effect of other major events.

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[15] In May 2001 Andersen paid $110 million to settle the suit with Sunbeam's shareholders. In this case Andersen did not deny the blame. In the case of Waste Management, Andersen agreed to pay a part of a $220 million class action suit. In the case of the Baptist Foundation of Arizona, Andersen agreed to pay $217 million (Yahoo! UK & Ireland News 2002; ClassActionAmerica.com 2002).

[16] Given the insurance right, the investors could recoup some of their losses from the deep pockets of the auditor. If the auditor is defunct, this right no longer exists.

[17] Andersen was the auditor for Enron, WorldCom, Qwest, Global Crossing, Dynegy, CMS Energy, Halliburton, Peregrine, and Merck at the time these companies allegedly used questionable accounting practices.

Ten firms were eliminated because their daily trading prices were not available in Yahoo! Finance or their trading prices were below \$0.25 per share, and 28 firms were removed because their financial data were unavailable from Compustat. Sixty firms announced quarterly earnings, dividend payments, management-related events, earnings forecasts/analyses, or financial distress during the five-day window surrounding the receipt date of sworn statements. Our final sample consists of 590 firms, of which 578 certified their statements by the deadline, while 12 either did not file statements by the deadline or filed a different form of statement than required by the SEC, classified by the SEC as "All Others."[19] Panel A of Table 1 reports the sample attrition due to these reasons. Panel B reports the distribution of the sample firms by two-digit SIC code. The number of sample firms in each industry ranges from 1 to 65.

Event Window

While some sample firms announced their intentions to certify by August 14, 2002 on their websites or in press releases, others did not. Therefore, our sample consists of two groups: (1) firms with website/press release dates earlier than or the same day as the SEC website receipt dates (PRESS sample); and (2) firms without website/press release or release dates later than the SEC official website release dates (SEC sample). For the PRESS sample, the two-day event window is defined as days (0, +1). For the SEC sample, the two-day event window is defined as days (+1, +2), based on our frequent monitoring of the SEC website and our discussion with the SEC webmaster. The receipt date shown on the SEC website differed from the date the SEC actually posted the information on its website. For example, although the receipt date shown on the SEC website was August 12, 2002, the information was actually posted on the website one or two days later. Delays were caused by the complicated workflow of the SEC in dealing with certification documents and the fact that a vast majority of firms certified close to August 14, 2002.[20] Therefore, for the SEC sample, we excluded the receipt day (day 0) from the calculation of cumulative abnormal returns (*CAR*) on the basis of our observation that before August 13, 2002, the SEC updated the filing status at least one day after it received statements, and it posted the information almost two days after receipt for firms that filed their statements on August 13 and 14, 2002.

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[18] We eliminated four firms, LTV Corp., Adelphia, Enron Corp., and Act Manufacturing, which had stock prices less than \$0.25 and were not actively traded. Using the Menon and Williams (1994) procedure we eliminated any confounding effect due to earnings announcements. We also removed all firms affected by confounding events as identified by Thompson et al. (1987).

[19] Four firms that did not file or filed different forms were eliminated during the sample selection procedure.

[20] The SEC webmaster described to us that the workflow was approximately as follows: (1) certifications received by Office of the Secretary staff: date stamped, tracking numbers assigned, date and number recorded in spreadsheet; (2) certifications scanned into PDF by Desktop Publishing (originals retained on file); (3) PDFs placed into holding directory on network; (4) PDFs copied (more or less simultaneously) by Division of Corporation Finance (for review) and by Branch of Internet Publishing/Office of the Secretary (for website posting); (5) PDFs posted to website while Corporation Finance review commences; (6) after Corporation Finance review is complete, Corporation Finance passes status ("Exhibit-A form" versus "all other") to Internet Publishing; (7) Internet Publishing adds status to website (by adding checkmark to correct column).

TABLE 1
Sample Firms Used in the Study

Panel A: Sample Selection

Initial sample[a]	688
Less: Stock prices less than $0.25 or not available from Yahoo! Finance	10
Less: Financial information not available from Compustat	28
Less: Firms with confounding events[b]	60
Final sample	590
Classification of final sample	
Statement in exhibit A form filing by August 14, 2002	578
All others[c]	12

Panel B: Distribution of Sample Firms by Industry

Two-Digit SIC Codes	Name of Industry	Number of Firms
01	Agricultural Production—Crops	1
07	Agricultural Services	1
10	Metal, Mining	2
12	Coal Mining	2
13	Oil and Gas Extraction	13
14	Nonmetallic Minerals, except Fuels	2
15	General Building Contractors	9
16	Heavy Construction, Except Building	3
17	Special Trade Contractors	2
20	Food and Kindred Products	11
21	Tobacco Products	3
22	Textile Mill Products	2
23	Apparel and Other Textile Products	2
24	Lumber and Wood Products	3
25	Furniture and Fixtures	5
26	Paper and Allied Products	14
27	Printing and Publishing	12
28	Chemical and Allied Products	42
29	Petroleum and Coal Products	10
30	Rubber and Miscellaneous Plastics Products	4
32	Stone, Clay, and Glass Products	2
33	Primary Metal Industries	8
34	Fabricated Metal Products	9
34	Industrial Machinery and Equipment	30
36	Electronic and Other Electric Equipment	26
37	Transportation Equipment	25
38	Instruments and Related Products	13
39	Miscellaneous Manufacturing Industries	3
40	Railroad Transportation	4
42	Trucking and Warehousing	7
45	Transportation by Air	9

(continued on next page)

TABLE 1 (Continued)

Two-Digit SIC Codes	Name of Industry	Number of Firms
47	Transportation Services	2
48	Communications	20
49	Electric, Gas and Sanitary Services	65
50	Wholesale Trade—Durable Goods	16
51	Wholesale Trade—Non-durable Goods	8
52	Building Materials and Gardening Supplies	1
53	General Merchandise Stores	2
54	Food Stores	2
55	Automotive Dealers and Service Stations	6
56	Apparel and Accessory Stores	2
57	Furniture and Home Furnishings Stores	1
58	Eating and Drinking Places	4
59	Miscellaneous Retail	8
60	Depository Institutions	40
61	Non-depository Institutions	8
62	Security and Commodity Brokers	5
63	Insurance Carriers	40
64	Insurance Agents, Brokers, and Service	2
67	Holding and Other Investment Offices	7
70	Hotels and Other Lodging Places	3
72	Personal Services	1
73	Business Services	37
75	Auto Repair, Services, and Parking	2
78	Motion Pictures	2
79	Amusement and Recreation Services	4
80	Health Services	13
87	Engineering and Management Services	5
99	Nonclassifiable Establishments	5
Total		590

[a] Firms required to file statements by August 14, 2002.
[b] See Thompson et al. (1987) for the identification of confounding events.
[c] Includes firms not filing a statement by August 14, 2002, or filing a different form of statement than required by the SEC.

Measurement of Variables

Market Reaction and Portfolio Returns

In a typical event study, it is assumed that abnormal returns are uncorrelated across firms. In this case, however, we have partial clustering of events that may cause the cross-correlation problem described in Bernard (1987). To mitigate the impact of the cross-correlation caused by temporal events clustering, we use the portfolio method suggested by Sefcik and Thompson (1986) to test market reaction to certification.[21] Following Baber et al. (1995), we specify the following portfolio-based estimation model to investigate the overall market reaction to the certification:

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[21] The portfolio method has been used by many prior studies including Jaffe (1974), Brown and Warner (1980), Schipper and Thompson (1983), Binder (1985), Bernard (1987), Sefcik and Thompson (1986), and Baber et al. (1995).

$$R_{pt} = \alpha_p + \beta_p \cdot R_{mt} + \sum_j \gamma_{pj} \cdot D_{jt} + e_{pt} \tag{1}$$

where R_{pt} is the return on an equally weighted portfolio p for day t, α_p is the intercept term, R_{mt} is the S&P 500 return for day t, β_p is the market return slope coefficient, γ_{pj} is an event parameter capturing the impact of the event on portfolio p's returns (i.e., the abnormal return of the jth event day), $\sum_j \gamma_{pj}$ is the cumulative abnormal return over the appropriate event period (i.e., for all j in the event period), D_{jt} is a dummy variable equal to 1 if day t is the jth event day (i.e., $t = j$) and 0 otherwise, and e_{pt} is a random disturbance term.

The estimation period for model (1) begins 90 trading days prior to the certification and lasts for 90 trading days after the certification including day 0. The abnormal portfolio return of the jth event day, $AR_{PORT}(j) = \gamma_{pj}$, and the cumulative abnormal portfolio return over the two-day event period, $CAR_{PORT} = \sum_j \gamma_{pj}$, are based on Equation (1).

We use a time-series of daily portfolio returns of an equally weighted portfolio of certifying firms created on each certification date to obtain parameter estimates from Equation (1). We create four portfolios for each of the two subsamples, totaling eight portfolios. Each portfolio consists of at least 25 observations to lessen the concern regarding the power of the test with smaller sample size.[22] The returns of this time-series portfolio fully account for the cross-correlation in individual firm disturbance and, hence, the cross-correlation problem caused by event clustering is diminished (Sefcik and Thompson 1986; Baber at al. 1995).

Market Reaction and Individual Firm Abnormal Returns

Abnormal return on day t is determined as the difference between the actual returns and the expected returns derived from the market model:

$$AR_{it} = R_{it} - (\hat{\alpha}_i + \hat{\beta}_i R_{mt}) \tag{2}$$

where:

R_{it} = return for firm i on day t;
α_i = intercept;
β_i = beta for firm i;
R_{mt} = return on the value-weighted S&P 500 Index on day t; and both α_i and β_i are estimated over the 90 day event window (−94, −5).

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[22] It is simply by chance that there were only four days (8/14, 8/13, 8/12, and 8/9) having 25 or more firms reporting certifications on the same day in both the PRESS and SEC samples.

Market reaction is defined as the cumulative abnormal return (*CAR*) during the two-day event period (+1, +2).[23] We used the S&P 500 Index as the market index to estimate the market model. The average (median) market capitalization for our sample was \$12.5 billion (\$3.8 billion), and the average (median) market capitalization for the S&P 500 was \$16.8 billion (\$7 billion) as of July 31, 2004.

Bid-Ask Spread

In addition to measuring the impact of certification by analyzing market returns, we examine bid-ask spreads for the certifiers before and after certification. Because certification was intended to provide assurance to investors, certification announcements have the potential to reduce information asymmetry. We specify the following regression model to estimate bid-ask spread behavior:

$$Spread = \beta_0 + \beta_1 LogVolume + \beta_2 ReturnVariability + \beta_3 Evnt + \varepsilon \tag{3}$$

where:

$Spread$ = $2*(ASK - BID)/(ASK + BID)$;
$LogVolume$ = the logarithm of the total number of shares traded;
$ReturnVariability$ = the square of stock returns, a proxy for return variability; and
$Evnt$ = a dummy variable, taking the value of 1 if the trading day falls in the event window (0, + 89), and 0 if it falls in the event window (−90, −1).[24]

The coefficient for *Evnt* is used to examine whether there are changes in the spread before and after certifications that are not accounted for by trading volumes or by return variability, two control variables for bid-ask spread (e.g., Stoll 1989; Easley and O'Hara 1987; Kim and Verrecchia 1994). A significantly negative coefficient of the dummy variable *Evnt* would suggest a significant reduction in the bid-ask spread after certification.

Explanatory Variables for Cross-Sectional Analysis

RevenueGrowth: We define *RevenueGrowth* as the difference between current year revenue (2001) and previous year revenue (2000) divided by previous year revenue (2000). Following Chaney and Philipich (2002), we use *RevenueGrowth* as a proxy for aggressive revenue recognition, due to its importance in accounting irregularities associated with firms like Enron and WorldCom. As discussed in Section II, in this context higher revenue growth implies aggressive accounting reporting practices. That is, investors are uncertain about whether the higher revenue growth is from either doubtful revenue recognition or actual revenue growth. We hypothesize in H3 that *RevenueGrowth* is positively associated with cumulative abnormal returns.

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[23] Prior research by Brown and Warner (1985), Bernard (1987), and Salinger (1992) suggests that the bias caused by cross-sectional correlations in OLS regressions of *CAR* as the dependent variable is inconsequential when using daily return and short-event windows. Brown and Warner (1985) further note that tests ignoring cross-sectional dependence can be well specified and have higher power than tests that account for potential dependence when using daily return data. Since we use daily returns to measure *CAR*s over short-event windows, like other event studies by Menon and Williams (1994) and Chaney and Philipich (2002), we do not expect such a bias to exist in our estimation or to affect the inference of our results.

[24] We also estimated the bid-ask spread for the period beginning 120 trading days prior to the certification and lasting for 120 trading days after the certification. The results remained unchanged.

Investigation: This is a dummy variable equal to 1 if the firm is under investigation by the SEC and the Department of Justice, and 0 otherwise. Our fourth hypothesis (H4) predicts a positive association between this variable and abnormal return.

Andersen: This is a dummy variable equal to 1 if the firm is one of Andersen's clients, and 0 otherwise. Our fifth hypothesis (H5) predicts a positive association between this variable and abnormal return.

Control Variables for Cross-Sectional Analysis

Nine control variables are included. We have no prediction regarding their signs, but we include them to mitigate problems of potentially omitted correlated variables. These nine variables are:

TIME: The number of trading days counting back from August 14, 2002, to the date when the CEO/CFO statement was received. August 14, 2002, the deadline for submission, is coded as 1. Prior studies have shown that firms prefer to announce good news earlier, and that investors react negatively if an earnings release is delayed (Chambers and Penman 1984). However, in our case, since the deadline was August 14, 200, and firms were allowed to certify any time up to that date, it is unclear how investors would react to firms that certified earlier or later, as long as they certified by the deadline.[25] Therefore, we included *TIME* as a control variable.

lnAssets: Logarithm of total assets measured in millions of dollars, used to control for firm size.[26]

BM: The ratio of the book value per share to the market price per share is used to control for the growth potential of the firm (Gaver and Gaver 1993).

Leverage: Long-term debt divided by total assets is used to control for the potential impact of leverage on abnormal returns (DeFond and Jiambalvo 1991).

Big5: A dummy variable equal to 1 if the firm's auditor is one of the then Big 5 accounting firms, and 0 otherwise.

Committee: A dummy variable equal to 1 if the certification indicated that the CEO/CFO reviewed the financial statements with the firm's audit committee, and 0 otherwise.

Audited-Q: A dummy variable equal to 1 if the firm's 10-Q is audited by its auditor, and 0 otherwise.[27]

Regulated: A dummy variable equal to 1 if the firm's SIC is between 4000–4999 (Utility industry), and 0 otherwise.

Financials: A dummy variable equal to 1 if the firm's SIC is between 6000–6999 (e.g., it is a financial institution such as a bank or an insurance company), and 0 otherwise.

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[25] One can argue that firms certifying earlier have a transparent reporting process or better reporting system and are confident about their numbers. Similarly, firms may certify later due to thoroughly evaluating their numbers before certifying them. Hence, both of these reporting schedules can be viewed by investors as positive or nonevents.

[26] A non-exhaustive list of studies using firm size as a control includes Banz (1981), Reinganum (1981), Atiase (1985), Lo and MacKinlay (1990), Fama and French (1992), and Brennan et al. (1993).

[27] It is possible that nonaudited firms may benefit more from certification than audited firms. For details, please see Ettredge et al. (1994).

Regression Model for Cross-Sectional Analysis

To test hypotheses H3–H5, we estimate the following regression model:

$$\begin{aligned} CAR = {} & \gamma_0 + \gamma_1 RevenueGrowth + \gamma_2 Investigation + \gamma_3 Andersen + \gamma_4 TIME \\ & + \gamma_5 lnAssets + \gamma_6 BM + \gamma_7 Leverage + \gamma_8 Big5 + \gamma_9 Committee \\ & + \gamma_{10} Audited\text{-}Q + \gamma_{11} Regulated + \gamma_{12} Financials + \varepsilon \end{aligned} \tag{4}$$

where *CAR* is the cumulative abnormal returns during the two-day event period as defined earlier, the independent variables are as described above, and ε is a random disturbance term.

IV. EMPIRICAL RESULTS AND DISCUSSION

Descriptive Statistics

Table 2 presents descriptive statistics of variables in the regression model. For 578 certifiers, the mean *CAR* during the two-day event period was 0.7 percent, and the mean annual revenue growth was 9.5 percent. Ten firms were under investigation even though they filed the required sworn statements by the deadline, 115 firms were audited by Andersen, and 15 firms had their 10-Q reports audited. There were 12 All Others firms that failed to file or filed a different form than required by the SEC. Further, the sample firms filed the sworn statements about four trading days, on average, prior to the deadline.

We report Pearson and Spearman correlations between the independent variables of the regression model (Equation (4)) in Table 3. The correlation coefficient between *RevenueGrowth* and *Investigation* is significantly positive, suggesting that firms under investigation by the SEC and/or the Department of Justice tend to report higher revenue growth. In contrast, the correlation coefficient between *Andersen* and *TIME* is significantly negative, indicating that firms audited by Andersen did not file their CEOs'/CFOs' sworn statements as early as those not audited by Andersen.

Portfolio and Individual Firm Returns

Abnormal returns and cumulative abnormal returns based on the portfolio method around event dates are reported in Table 4.[28] Panel A shows that the cumulative abnormal portfolio returns for firms in the PRESS sample were significantly positive on August 9, 12, and 13, 2002. Similarly, Panel B shows that the cumulative abnormal portfolio returns for firms in the SEC sample were significantly positive on August 12, 13, and 14, 2002, indicating a significant impact of the sworn certification on abnormal portfolio returns during the event period. The percentage of positive abnormal returns estimated from the market model ranges from 57.3 percent to 66.6 percent and is statistically significantly different from 50 percent for all dates except August 14, 2002, in the PRESS sample and August 9, 2002, in the SEC sample. Overall, the results are consistent with our first hypothesis.

Results of Bid-Ask Spread Estimation

We present the results of the bid-ask spread regression for the certification of financial statements in Table 5. Consistent with our second hypothesis, the coefficient for the dummy variable *Evnt* is negative and statistically significant at the 1 percent level, indicating that the spread narrows in the post-certification period.

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[28] The results remained unchanged when firms under investigation were excluded from the estimation.

TABLE 2
Descriptive Statistics of Selective Variables

	Certified Firms In Exhibit A Form (n = 578)		All Others[a] (n = 12)		Total (n = 590)	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
CAR	0.007	0.042	−0.001	0.015	0.007	0.043
RevenueGrowth	0.095	0.412	0.470	0.627	0.103	0.420
Investigation	0.017	0.130	0.083	0.288	0.019	0.135
Andersen	0.199	0.399	0.333	0.492	0.202	0.401
TIME	3.847	3.694	1.666	1.345	3.809	3.677
lnAssets	8.791	1.636	8.890	1.658	8.793	1.637
BM	0.507	1.798	1.053	1.711	0.518	1.798
Leverage	0.266	0.196	0.366	0.179	0.268	0.196
Big5	0.590	0.510	0.500	0.522	0.588	0.505
Committee	0.965	0.182	0.750	0.452	0.961	0.193
Audited-Q	0.026	0.160	0	0	0.025	0.157
Regulated	0.179	0.383	0.250	0.452	0.181	0.385
Financials	0.176	0.381	0	0	0.173	0.378

[a] Includes firms not filing a statement by August 14, 2002, or filing a different form of statement than required by the SEC.

CAR = cumulative abnormal returns during the two-day event period subsequent to the SEC receipt date of sworn statements or including the date of and the date subsequent to the press release date;
RevenueGrowth = difference between current and previous year's revenues, divided by previous year's revenues;
Investigation = dummy variable, equal to 1 if the firm is under investigation by the SEC or the Department of Justice and files required statement;
Andersen = dummy variable, equal to 1 if the firm is one of Andersen's clients in 2001;
TIME = number of days counting back from August 14, 2002 (with August 14 coded as 1), to the date on which the CEO/CFO statement was received;
lnAssets = logarithm of total assets measured in millions of dollars ($M);
BM = book value per share/market price per share;
Leverage = long-term debt divided by total assets;
Big5 = dummy variable, equal to 1 if the firm is a client of one of the Big 5 firms in 2001;
Committee = dummy variable, equal to 1 if the certification indicated that the firm's financial statements were reviewed by its audit committee;
Audited-Q = dummy variable, equal to 1 if the firm's 10-Q was audited by its auditor;
Regulated = dummy variable, equal to 1 if the firm's SIC is between 4000–4999 (Utility industry); and
Financials = dummy variable, equal to 1 if the firm's SIC is between 6000–6999 (Financials).

Regression Results for Cross-Sectional Analysis

We use OLS regressions for the PRESS sample and the SEC sample separately and for both groups combined. Table 6 presents the results. Consistent with our third hypothesis, the coefficients for *RevenueGrowth* are significantly positive for all three regressions, indicating that the firms with larger revenue growth benefit more from CEOs'/CFOs' certifications.[29] Similarly, consistent with our fourth hypothesis, the coefficients for *Investigation* are positive and statistically significant at conventional levels for all three regressions, suggesting that certifications provide a greater level of assurance for firms under investigation. Consistent with our fifth hypothesis, the coefficients on *Andersen* are significantly positive,

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[29] The results are robust when we employ another metric, *RevenueGrowth* deflated by its lagged measure.

TABLE 3
Correlation Matrix for Independent Variables
(n = 578)

	Revenue Growth	Investigation	Andersen	TIME	lnAssets	BM	Leverage	Big5	Committee	Audited-Q	Regulated	Financials
RevenueGrowth	1.000	0.084*	0.014	−0.029	0.101*	0.040	0.083*	−0.005	0.028	−0.066	0.101*	0.020
Investigation	0.104*	1.000	0.022	−0.018	0.207*	0.025	−0.030	0.019	0.022	−0.022	−0.058	0.076
Andersen	0.016	0.031	1.000	−0.077*	−0.056	0.033	0.113*	0.032	−0.039	−0.031	0.112*	−0.122*
TIME	−0.031	−0.015	−0.097*	1.000	0.002	0.035	−0.033	−0.029	−0.022	0.014	0.030	−0.031
LnAssets	0.131*	0.226*	−0.086*	0.000	1.000	0.018	−0.074*	−0.032	0.047	0.064	0.097*	0.128*
BM	0.038	0.015	0.024	0.036	0.019	1.000	−0.107*	0.003	−0.005	0.001	0.030	0.047
Leverage	0.093*	−0.037	0.154*	−0.049	−0.084*	−0.112*	1.000	0.005	−0.029	0.012	0.139*	−0.203*
Big5	−0.006	0.011	0.042	−0.032	−0.036	0.008	0.006	1.000	0.006	0.014	0.015	0.016
Committee	0.032	0.013	−0.033	−0.019	0.046	−0.003	−0.037	0.008	1.000	0.017	−0.004	−0.038
Audited-Q	−0.056	−0.020	−0.037	0.012	0.064	0.002	0.014	0.013	0.015	1.000	−0.020	0.057
Regulated	0.087*	−0.060*	0.125*	0.028	0.110*	0.041	0.160*	0.017	−0.002	−0.021	1.000	−0.193*
Financials	0.020	0.076*	−0.147*	−0.030	0.469*	0.045	−0.263*	0.015	−0.047	0.059	−0.213	1.000

* Significant at the 10 percent level using two-tailed tests.

Pearson correlations are below the diagonal, and Spearman correlations are above the diagonal.

RevenueGrowth = difference between current and previous year's revenues, divided by previous year's revenues;
Investigation = dummy variable, equal to 1 if the firm is under investigation by the SEC or the Department of Justice and files required statement;
Andersen = dummy variable, equal to 1 if the firm is one of Andersen's clients in 2001;
TIME = number of days counting back from August 14, 2002 (with August 14 coded as 1), to the date whichever the CEO/CFO statement was received;
lnAssets = logarithm of total assets measured in millions of dollars ($M);
BM = book value per share/market price per share;
Leverage = long-term debt divided by total assets;
Big5 = dummy variable, equal to 1 if the firm is audited by one of the Big 5 firms in 2001;
Committee = dummy variable, equal to 1 if the firm's financial statements were reviewed by its audit committee;
Audited-Q = dummy variable, equal to 1 if the firm's 10-Q was audited by its auditor;
Regulated = dummy variable, equal to 1 if the firm's SIC is between 4000–4999 (Utility industry); and
Financials = dummy variable, equal to 1 if the firm's SIC is between 6000–6999 (Financials).

TABLE 4

Market Reaction around the Press Release Dates or Receipt Dates of Sworn Statements

Panel A: Certifying Firms in the PRESS Sample

		Portfolio Method			**Market Model**
Dates	**n**	$AR_{port}(0)$	$AR_{port}(+1)$	$CAR_{port}(0,+1)$	**Percentage of Positive *CAR***
8/14	75	−0.0027	0.0096*	0.0069	57.3%
		(−0.453)	(1.606)	(0.952)	(1.270)
8/13	60	0.0035	0.0112**	0.0147**	66.6%***
		(0.805)	(1.961)	(1.692)	(2.582)
8/12	37	0.0060*	0.0062*	0.0122*	62.2%*
		(1.574)	(1.603)	(1.342)	(1.480)
8/9	30	0.0050	0.0055	0.0105*	63.3%*
		(1.159)	(1.236)	(1.292)	(1.461)
Panel B: Certifying Firms in the SEC Sample					
8/14	103	0.0053	0.0046	0.0099*	58.3%**
		(0.890)	(0.841)	(1.312)	(1.675)
8/13	103	0.0078**	0.0025	0.0103*	57.3%*
		(1.782)	(0.650)	(1.329)	(1.478)
8/12	40	0.0082**	0.0037	0.0116*	62.5%*
		(1.963)	(0.826)	(1.368)	(1.581)
8/9	31	0.0030	0.0050	0.0080	61.2%
		(0.567)	(0.904)	(1.160)	(1.257)

*, **, *** Significant at the 10 percent, 5 percent, and 1 percent levels, respectively, using one-tailed tests.

$AR_{PORT}(j)$ = abnormal returns for an event day *j* based on the portfolio method;

CAR_{PORT} = cumulative abnormal returns over the two-day event period based on the portfolio method; and

CAR = cumulative abnormal returns over the two-day event period based on the market model.

indicating that the market reacted favorably to firms audited by Andersen if they filed statements by the deadline. The coefficients for all the control variables are insignificant across the three regressions, indicating that these variables did not impact cumulative abnormal returns.

Additional Analysis and Sensitivity Check

All Others versus Certifiers

Of the 688 companies whose CEOs and CFOs had to swear to the accuracy of their financial statements by August 14, 2002, the SEC flagged 16 companies for failure to adequately vouch for their financial statements. Many of these noncomplying companies were widely known to have accounting problems. The SEC put these flagged companies under the All Others heading. One company called IT Group did not file its financial statements nor request an extension. The vast majority of certifying companies stood by their previously reported numbers. Undoubtedly, the noncomplying companies sent a negative signal to investors about their financial numbers. To evaluate the difference in the abnormal returns between all others and certifiers, we use a matched-pair design methodology. Specifically, we construct a portfolio of 12 firms that failed to file or filed different forms of certifications as the All Others experimental group, and we matched that portfolio

TABLE 5
Regression Results of Bid-Ask Spread for Certifying Firms

$$Spread = \beta_0 + \beta_1 Log\ Volume + \beta_2 Return\ Variability + \beta_3 Evnt + \varepsilon$$
(n = 578)

Variables	Coefficient	Predicted Signs	Coefficients (t-stat.)
Constant	β_0		−0.0037*** (−3.302)
Log Volume	β_1		0.0083*** (42.873)
Return Variability	β_2		2.219*** (149.233)
Evnt	β_3	—	−0.0055*** (−19.925)

Adj. R^2 = 32.8%

*** Significant at the 1 percent level, using two-tailed tests.

Spread = 2*(*ASK* − *BID*)/(*ASK* + *BID*);
Log Volume = the log of the total number of shares traded in the company;
Return Variability = the square of stock return, a proxy for return variability; and
Evnt = a dummy variable equal to 1 if *t* lies in the event window (0, + 89) and 0 if *t* lies in the event window (−90, −1).

with a portfolio of 12 certifying firms in the same industries with similar total assets. Table 7 reports the results of the statistical test for differences in the mean cumulative abnormal returns between All Others and certifiers. It is evident that the cumulative abnormal return of All Others is significantly smaller than that of the peer group of certifiers.

We also estimated the bid-ask spreads for non-certifying firms. The results appear in Panel A of Table 8. The coefficient for *Evnt* is negative but insignificant for non-certifiers. To compare the bid-ask spreads of non-certifying firms with certifying firms, we include both non-certifying and certifying firms in our bid-ask spreads estimation model, and introduce two variables (a dummy variable, *Certifier*, and an interaction term, *Evnt* × *Certifier*), to capture the potential differences in bid-ask spreads between the certifying and non-certifying firms. The coefficients of *Certifier* and *Evnt* × *Certifier*, which are reported in Panel B of Table 8, are both significantly negative indicating a substantial reduction in bid-ask spreads for certifying firms compared to non-certifying firms.

Sensitivity Checks

To evaluate the robustness of our regression results to an alternative event window specification, we use the *CAR* over the three-day event period as the dependent variable in our regression model (4). The untabulated results are consistent with those reported in Table 6.

To ensure that our inferences based on the regression analysis are reasonable, we examine the data for collinearity and the residuals for heteroscedasticity. The condition indices and proportions of variation in the eigenvector-based diagnostics as set out in Belsley et al. (1980) are well within acceptable limits. In addition, White's (1980) test procedure provides no evidence of heteroscedasticity. Finally, we use the regression diagnostics of

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TABLE 6
OLS Regression Results

$$CAR = \gamma_0 + \gamma_1 RevenueGrowth + \gamma_2 Investigation + \gamma_3 Andersen + \gamma_4 TIME + \gamma_5 lnAssets + \gamma_6 BM + \gamma_7 Leverage + \gamma_8 Big5 + \gamma_9 Committee + \gamma_{10} Audited\text{-}Q + \gamma_{11} Regulated + \gamma_{12} Financials + \varepsilon$$

Variables	**Coefficient**	**Predicted Signs**	**The PRESS Sample (n = 243)**	**The SEC Sample (n = 335)**	**Combined Sample (n = 578)**
Intercept	γ_0		−0.0327 (−0.413)	0.0012 (0.271)	0.0165 (0.535)
RevenueGrowth	γ_1	+	0.0118** (1.741)	0.0124** (1.996)	0.0097** (2.154)
Investigation	γ_2	+	0.0220* (1.307)	0.0777*** (2.720)	0.0361*** (2.507)
Andersen	γ_3	+	0.0100* (1.418)	0.0089* (1.601)	0.0068* (1.485)
TIME	γ_4		0.0012 (1.135)	0.0002 (0.392)	0.0004 (0.946)
lnAssets	γ_5		0.0002 (0.037)	−0.0047 (−1.164)	−0.0031 (−0.941)
BM	γ_6		0.0008 (0.601)	0.0044 (1.202)	0.0013 (1.224)
Leverage	γ_7		0.0085 (0.540)	−0.0065 (−0.517)	−0.0026 (−0.272)
Big5	γ_8		0.0314 (0.902)	−0.0383* (−1.301)	−0.0104 (−0.481)
Committee	γ_9		−0.0032 (−0.080)	0.0125 (0.695)	0.0045 (0.255)
Audited-Q	γ_{10}		−0.0117 (−0.828)	0.0040 (0.199)	−0.0070 (−0.633)
Regulated	γ_{11}		0.0033 (0.374)	−0.0031 (−0.468)	−0.0017 (−0.368)
Financials	γ_{12}		0.0166 (1.045)	0.0070 (1.155)	0.0210 (1.037)
Adjusted R^2			0.018	0.019	0.040

*, **, *** Significant at the 10 percent, 5 percent, and 1 percent levels, respectively, using one-tailed tests.

CAR = cumulative abnormal returns during the two-day event period;
RevenueGrowth = difference between current and previous year's revenues, divided by previous year's revenue;
Investigation = dummy variable, equal to 1 if the firm is under investigation by the SEC or the Department of Justice and files required statement;
Andersen = dummy variable, equal to 1 if the firm is one of Andersen's clients in 2001;
TIME = number of days counting back from August 14, 2002 (with August 14 coded as 1), to the date whichever the CEO/CFO statement was received;
lnAssets = logarithm of total assets measured in millions of dollars ($M);
BM = book value per share/market price per share;
Leverage = long-term debt divided by total assets;
Big5 = dummy variable, equal to 1 if the firm is audited by one of the Big 5 firms in 2001;
Committee = dummy variable, equal to 1 if the firm's financial statements were reviewed by its audit committee;
Audited-Q = dummy variable, equal to 1 if the firm's 10-Q was audited by its auditor;
Regulated = dummy variable, equal to 1 if the firm's is SIC between 4000–4999 (Utility industry); and
Financials = dummy variable, equal to 1 if the firm's SIC is between 6000–6999 (Financials).

TABLE 7
Statistical Test Results of Equality of Mean *CAR* between All Others and Certifiers Based on Industry and Firm Size

CAR (Certifiers, n = 12)	*CAR* (All Others, n = 12)	Mean Difference	Welch Test: t-statistic	Wilcoxon Test: z-statistic
0.0024	−0.0012	0.0036	1.430*	−1.559*

* Significant at the 10 percent level, using one-tailed tests.
CAR = cumulative abnormal returns over the two-day event period.

Belsley et al. (1980) to remove influential observations and we re-estimate our models.[30] After removing nine observations from the estimation, we obtain qualitatively unchanged results.

V. SUMMARY AND CONCLUSION

We study the impact of the SEC order issued on June 27, 2002, on share prices of firms required by the SEC to certify their financial statements. The SEC issued an administrative order requiring CEOs and CFOs of 688 publicly traded U.S. companies to file sworn statements regarding the accuracy and completeness of their companies' most recently filed financial statements by August 14, 2002. Our first two hypotheses are based on the fact that certification has an assurance value and decreases the information asymmetry between managers and owners regarding a firm's value by making disclosures more credible. Our empirical results are consistent with our hypotheses. We hypothesize that the magnitude of cumulative abnormal returns during the event window is positively related to revenue growth, investigation status, and being an Andersen client. Once again, the findings are consistent with our hypotheses.

Taken together, our results suggest that the SEC order requiring filing of sworn statements by CEOs and CFOs had a positive effect on the market value of certifying firms, consistent with the notion that certification improved investors' confidence in corporate disclosures. It is conceivable that enacting the sworn certification of financial statements as a permanent reporting requirement in the Sarbanes-Oxley Act of 2002 decreased investor mistrust of corporate disclosures.

APPENDIX
List of Sample Firms under Investigation by the SEC and the Department of Justice

AOL Time Warner
Issue: Did the multimedia company inflate revenue?

Bristol-Myers Squibb
Issue: The company allegedly inflated sales by $1 billion in 2001.

Citigroup Inc.
Issue: Did off-shore units it controlled allow Enron to inflate revenue?

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[30] Specifically, we remove observations from the estimation if at least one of their DFFITS and DFBETAS (generated in SAS) exceeds the general cutoff value of 2.

TABLE 8
Regression Results of Bid-Ask Spread

Panel A: Non-Certifying Firms (n = 12)

Variables	Coefficient	Predicted Signs	Coefficients (t-stat.)
Constant	β_0		−0.284*** (−10.992)
Log Volume	β_1		0.0576*** (14.142)
Return Variability	β_2		0.339*** (5.493)
Evnt	β_3	−	−0.0045 (−0.833)
Adj. R^2 = 38.5%			
Panel B: All Sample Firms (n = 590)			
Constant	β_0		0.0089*** (3.972)
Log Volume	β_1		0.0087*** (43.710)
Return Variability	β_2		1.9980*** (140.743)
Evnt	β_3	−	0.0027 (0.903)
Certifier	β_4	−	−0.0146*** (−7.821)
Evnt × *Certifier*	β_5	−	−0.0082*** (−2.777)
Adj. R^2 = 31.1%			

*** Significant at the 1 percent level, using two-tailed tests.

Spread = 2*(*ASK* − *BID*)/(*ASK* + *BID*);
Log Volume = the log of the total number of shares traded in the company;
Return Variability = square of stock return, a proxy for return variability;
Evnt = a dummy variable taking the value of 1 if *t* lies in the event window (0, +89) and 0 if *t* lies in the event window (−90, −1); and
Certifier = a dummy variable taking the value of 1 if the firm is a certifier.

CMS Energy Corp.
Issue: Did Dearborn, Mich., firm use sham energy "swaps" to inflate 2000 and 2001 operating revenue?

Computer Associates International Inc.
Issue: Did Islandia, a NY-based software company, inflate operating revenue after making a series of options grants to top executives?

Halliburton Co.
Issue: Did Dallas firm in 1998 begin counting as operating revenue up to $100 million a year in overruns, even before clients agreed to make the payments? U.S. Vice President Dick Cheney was then CEO.

J.P. Morgan Chase
Issue: Did off-shore units it controlled allow Enron to inflate revenue?

Merck
Issue: For reporting $12.4 billion in revenue (related to its Medco unit) during 1999–2002 that it never collected. The company reported the accounting irregularity to the SEC in April, but did not release the amount until July 8, 2002.

Merrill Lynch
Issue: Nation's largest broker touted stocks in companies that were investment banking clients. Aided Enron.

Reliant Energy
Issue: Is under investigation for the phony trades used to inflate its revenue and trading volumes.

Xerox
Issue: A $6 billion restatement of 2000–2001 earnings sparked federal probes.

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THE ACCOUNTING REVIEW
Vol. 81, No. 1
2006
pp. 29–47

Responsibility for Cost Management Hinders Learning to Avoid the Winner's Curse

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ABSTRACT: Errors in estimated product costs lead firms to win business that is unprofitable, because firms are more likely to win business when underestimated product costs lead them to bid below actual cost (Cooper et al. 1992; Hilton 2005). Feedback from repeated competitive bidding markets can teach people to bid well above estimated costs to avoid this winner's curse (Kagel 1995; Kagel and Levin 2002). We present experimental evidence that such learning is substantially hampered by sellers' sense of responsibility for the costs. This effect is consistent with psychological evidence that people tend to attribute bad outcomes to environmental factors out of their control, such as cost-estimation errors, and attribute good outcomes to their own skills, such as their ability to choose effective cost-management initiatives (Miller and Ross 1975; Zuckerman 1979). The results suggest that responsibility structures that combine pricing and production decisions may have unexpected drawbacks.

Data Availability: *Data are available from the authors on request.*

I. INTRODUCTION

Errors in estimated product costs lead firms to win business that is unprofitable because firms are more likely to win business when underestimated product costs lead them to bid below actual cost (Cooper et al. 1992; Hilton 2005).[1] Feedback from repeated competitive bidding markets can teach people to bid well above estimated costs to avoid this winner's curse (Kagel 1995; Kagel and Levin 2002). We present experimental evidence that such learning is substantially hampered by sellers' sense of responsibility for the costs, which affects the inferences they draw from market feedback. This effect is

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[1] One of the "hardball competition" tactics for which Stalk and Lachenauer (2004) provide anecdotal evidence is to encourage competitors into an unprofitable niche where they underestimate their costs.

Helpful comments have been provided by Mark Nelson, Tom Dyckman, and participants at accounting workshops at the Boston Area Research Colloquium, Cornell University, Harvard University, The Ohio State University, University of Waterloo, and University of Arkansas. Thanks to Tony Lednor and Christian Mastilak for research support, and the Johnson Graduate School of Management for funding.

Editor's note: This paper was accepted by Marlys Lipe, Editor.

Submitted November 2004
Accepted July 2005

consistent with psychological evidence that people tend to attribute bad outcomes (losses) disproportionately to environmental factors out of their control, while attributing good outcomes disproportionately to their own skills, such as (in the responsibility condition) ability to manage costs effectively. The results suggest that firms' ability to respond to market feedback might vary with the responsibility structure of the organization. Some organizations assign responsibilities by function, separating pricing (marketing) from cost-management (production) responsibilities, while other organizations assign responsibilities by product line or region, combining pricing and cost-management responsibilities. Our experimental evidence suggests the existence of unexpected side-effects of this responsibility-structure choice in a setting with high uncertainty about future costs and closely matched, but not identical, competitors.

In our experiment, sellers bid on contracts for large complex projects, based on an estimate of project costs that includes error. Sellers' firms undertake cost-management initiatives that are intended to increase their cost advantage compared to competitors. In one condition (responsibility for cost management), sellers choose among five initiatives, based on evidence of the initiatives' past success. In the other condition (no responsibility), sellers receive the same information about the initiatives' past success, but are assigned an initiative, as if the decision were made by another member of their firm.

Sellers are not told the parameters governing the distributions of actual costs and estimation errors. Thus, they must rely on market feedback to learn optimal behavior over 14 periods in each of three replications of the task. In both settings, the effects of initiative on actual cost are small relative to noise in the cost estimates, making the auction an "almost common-value" auction (Klemperer 1998). In this setting, all equilibrium bids include a large amount of "padding" to avoid the winner's curse, but those who believe they have a cost advantage should pad less than those who believe they have a cost disadvantage.

Our results show that the winner's curse is severe in early periods for sellers in both conditions, who earn negative profits on average. Sellers without responsibility for cost management learn to avoid the curse, increasing the mean padding of their bids per period by over 60 percent between the first five and last five periods of the session. By the later periods of the third replication they are no longer incurring statistically significant losses. In contrast, sellers with cost-management responsibilities show no sustained increase in mean padding of bids, and their losses remain statistically significant throughout the session.

Several aspects of our results are consistent with the presence of attribution biases that lead sellers to over-attribute success to internal factors (their own efforts or talents) and failure to external factors for which they are not responsible (Miller and Ross 1975). First, post-experiment debriefing data shows that those in the responsibility setting believe they have better competitive positions and less accurate cost estimates than those in the no-responsibility setting, consistent with the pattern of attribution error we predict. Moreover, those who believe they have better competitive positions pad less on average, even though they tend to assess higher levels of cost-estimation error; beliefs about competitive position have more influence on bid-padding than do beliefs about cost-estimation error. Finally, we show that insufficient mean padding in the responsibility condition can be traced directly to insufficient response to market feedback. Sellers with responsibility for cost management respond poorly to feedback about their own profits, showing no tendency to increase padding more after incurring larger losses themselves than after observing larger losses by others, although their own losses should be more informative about their own cost structure. In contrast, sellers without responsibility for cost management increase padding significantly more after incurring larger losses themselves.

Our results shed new light on the conditions under which sellers are likely to make appropriate adjustments to noisy estimated costs. Involvement in managing production is often expected to provide knowledge that aids individuals in responding to market feedback and adjusting production and pricing decisions to allow for cost-system error (Gupta and King 1997; Briers et al. 1999). Although involvement in production can produce beneficial knowledge, our results suggest that it can also have negative effects on individuals' cost-related judgments by inducing biases that hamper learning from experience.

Finally, we extend understanding of self-serving attribution biases in business settings. Recent research in finance has shown how self-serving attribution biases can lead financial-market traders to become overconfident with experience, and how overconfidence generates excess trading and patterns of over- and underreaction to information (Daniel et al. 1998; Gervais and Odean 2001; Barber and Odean 2002). Our study examines effects of similar biases in an organizational setting, showing how the biases can be exacerbated or mitigated by job design choices within the organization—in this case, by separating versus combining the tasks of cost management and bidding.

The remainder of the paper is organized as follows. Section II develops the hypothesis. Section III describes the experiment, Section IV presents the results, and Section V concludes the paper.

II. THEORY AND HYPOTHESIS

The Winner's Curse

Textbooks, cases, and practitioner literature on product costing contain numerous examples of the winner's curse, in which firms win market share but lose money on products or services that are priced too low because their cost is underestimated (Cooper 1989; Cooper et al. 1992; Turney and Ittner 1993; Hilton 2005).[2] The winner's curse arises when sellers fail to recognize that the bidder with the lowest cost estimate is likely to have the largest cost *under*estimate; hence, if they bid aggressively based on low cost estimates, they will win bids but lose money when actual costs exceed the estimates.

Sellers should not only bid lower when they receive lower cost estimates, because the cost estimates are informative, but also they should add enough padding to the cost estimate to allow for the error it is likely to include. The equilibrium strategy is to pad bids enough to make a satisfactory expected profit *conditional on winning the bid.* Because sellers are more likely to win the bid when the cost estimates on which they rely have large negative errors, they need to pad enough to cover *large* negative errors, not average errors. Because errors will be larger when the cost system predicts actual costs poorly, the equilibrium level of padding increases in the cost-estimation error relative to variation in actual costs (Milgrom and Weber 1982). A large body of research shows that sellers fail to pad bids enough (and buyers fail to shave bids enough) to eliminate the winner's curse completely (Kagel 1995).

Much of the research on the winner's curse examines common-value auctions, but most product markets are better described by "almost common value" auctions, in which sellers' costs are similar but not identical (Klemperer 1998). In this case, the optimal amount of padding depends on sellers' beliefs about their competitive position. Those who believe they have the worst competitive position (the highest costs) should pad bids the most. If their actual costs are typically higher than competitors' costs, then they are likely to win bids only when the cost estimates on which they rely include particularly large negative errors. For these sellers, padding enough to make a profit conditional on winning the bid means padding enough to cover *particularly* large underestimations of cost. In contrast, those with the best competitive position (the lowest costs) should pad bids the least. When they win a bid based on a cost estimate below competitors' estimates, it is likely that their cost estimate is low because they are actually the low-cost producers, not because they have severely underestimated their costs. Therefore, they do not need to add as much padding to their cost estimates to allow for error in the estimates that support winning bids.

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[2] Much of the this literature on pricing and product-costing error focuses only on bias in the cost estimates and ignores random error, while our experiment examines the effects of random error only. The effects of random error are relevant to accounting projects intended to reduce bias, however. As Datar and Gupta (1994) point out, accounting-system refinements designed to reduce bias can increase random error.

Managers can sometimes use market feedback to identify and adjust for the winner's curse. Cooper and Kaplan (1987, 211) describe a firm in which managers learned to adjust their prices to allow for cost-estimation error based on market experience, including observation of the behavior of competitors with similar actual costs. Based on this experience, managers used a rule-of-thumb for pricing that set the desired profit margin higher for products that were expected to have a larger negative cost-estimation error.

Market feedback is not always successful in eliminating or even reducing the winner's curse, however, and the factors that support or impede learning are not fully understood. Little learning to avoid the winner's curse occurs in two-party acquisition problems, as long as participants do not switch between buyer and seller roles (Ball et al. 1991; Foreman and Murnighan 1996). Significant learning often occurs in auctions (Lind and Plott 1991; Garvin and Kagel 1994; Foreman and Murnighan 1996), although it tends not to transfer well across different auctions (e.g., auctions with different numbers of participants [Kagel and Levin 1986]). The winner's curse has been shown to persist with professional bidders from the commercial construction industry as well as with student subjects (Dyer et al. 1989), and pricing consistent with the winner's curse has been identified in the art market (Goetzmann and Spiegel 1995) and in government contracting (Bilginsoy 2000).

Attribution Biases

Research in psychology has extensively documented self-serving attribution bias, i.e., individuals' tendency to take credit for success and attribute failure to factors out of their control (Miller and Ross 1975; Zuckerman 1979). In most winner's-curse experiments, participants have no influence over or responsibility for the value of the object for which they are bidding or the accuracy of the information given to them. Such experimental settings are analogous to firms in which production, accounting, and marketing responsibilities are separated, and the manager who sets product prices has no responsibility for managing or estimating production costs. In the present study, we contrast this setting with one in which the same individual has some responsibility for both managing production costs and setting prices, but still has no responsibility or control over the magnitude of error in cost estimates. Based on attribution theory, we predict that this combination of decision responsibilities will limit sellers' learning from market feedback to adjust for the error in reported product costs.

In our setting, success and failure (i.e., earning versus losing money) are determined by four factors: the error in the cost estimate, the seller's competitive cost advantage, his or her bidding strategy, and competitors' bidding strategies. Regardless of whether sellers in this setting have responsibility for cost management, errors in cost estimation and competitors' bidding strategies are external causes, to which losses are likely to be attributed, while the seller's own bidding strategy is an internal cause, to which successes can be attributed.

Poor competitive advantage in costs, however, is an internal cause for sellers with cost-management responsibility and an external cause for those without this responsibility. In consequence, sellers with responsibility for cost management will be less willing to attribute losses to high actual costs. Instead, they will believe that their selection of a cost-management initiative was successful in reducing their costs on average, but that they lose money sometimes on winning bids because of exceptionally large errors in their cost estimates.

If otherwise rational sellers overestimate their own cost-competitiveness, then they should pad too little, while if they overestimate error in cost estimates, they should pad too much. These two effects could offset each other, but we believe that the former effect will outweigh the latter, for two reasons. First, sellers who attribute their losses to cost-estimation error may interpret instances of high-magnitude error as "a string of bad luck," i.e., several extreme draws from an error distribution with only moderate variance, rather than an indicator of high error variance that would call for increased padding. Second, even if sellers do infer high error variance, their bidding strategies are likely to be relatively insensitive to this inference, since robust experimental evidence of the winner's curse in common-value settings with unbiased but noisy information shows that people do not easily understand that unbiased estimation errors call for more padding (Ball et al. 1991; Kagel 1995). As a result, we predict that sellers with responsibility for both bidding and cost management will learn to increase padding more slowly than those who have responsibility only for bidding.

> **H:** Compared to sellers who are responsible for only pricing decisions, sellers who are responsible for both pricing and cost-management decisions learn less well from market feedback to avoid the winner's curse.

III. METHOD

Overview and Experimental Design

The experiment was conducted using computerized laboratory markets in which 64 M.B.A. students participated as sellers in auctions for production jobs. Participants were recruited via email or by their professors.[3] Participants arrived to the laboratory in groups of 16 and were randomly assigned to cohorts of four sellers who competed to win contracts for large, complex production jobs. The random assignment of seats to cohorts prevented participants from identifying which of the other individuals in the room were members of their cohort. Each seller's firm had a cost-management initiative that might affect production costs. All costs and prices were denominated in laboratory dollars that were ultimately converted into cash.

We manipulated participants' sense of responsibility for their production costs by allowing participants in the eight cohorts randomly assigned to a "responsibility" setting to choose their own cost-management initiatives based on information about the initiatives' past performance. Participants in the remaining eight cohorts were in a "no-responsibility" setting and were assigned cost-management initiatives by the experimenter, but also saw the same information about the initiatives' past performance. We manipulated experience by allowing participants to bid in three "regimes" of 14 periods each. Each regime had a new set of five cost-management initiatives.

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[3] Many participants were enrolled in a course on the behavior of product and financial markets, but had no prior knowledge of the nature or goals of the experiment.

The experiment has a 2 × 2 × 3 × 14 mixed design, with cost-management responsibility (present or absent) and order of initiative sets[4] manipulated between cohorts, and regime (1, 2, and 3) and period within regime (1–14) manipulated within cohorts. Order of initiative sets had no significant effect in any of our analyses, and is not discussed further.

Actual Project Costs

Participants were told that their actual production cost each period included two components:

- A *base cost,* which was the same for all sellers in each period but differed from period to period "for a variety of reasons, such as the size and location of the project, changes in input prices, and so on."
- A *cost reduction* for each seller due to the cost-management initiative chosen by or assigned to the seller. This reduction remained the same through the 14 periods of a regime and was an absolute amount (e.g., $50) rather than a percentage of the base cost. When a new regime began, sellers chose or were assigned a new cost-management initiative that might result in a different amount of cost reduction. Sellers did not know which cost-management initiative other sellers in their cohort had chosen (or been assigned), but they knew it was possible for more than one seller to have the same initiative.

Participants did not know the parameters of the distributions from which these cost components were drawn but had to learn them from experience. Both distributions were uniform. The base cost ranged over the interval [400, 1000]. The cost reduction was quite similar across sellers and cost-management initiatives, uniformly distributed over the interval [45, 55].

Estimated Project Costs

Before bidding in each round, sellers received an estimate of their product cost. They were told that the estimate was just as likely to be above actual cost as below it, and that the estimation error would vary randomly across periods and sellers. They were also told that no seller would have error-free estimates of the actual cost of his or her projects, and that the errors in sellers' cost estimates were likely (though not certain) to differ from each other. Sellers were not told the actual range of cost-estimation errors, which was over the interval [–200, 200].

Our setting differs from prior experimental studies of the winner's curse in two ways. First, most studies of the winner's curse have participants bid for an asset with a common cost or value (Lind and Plott 1991; Foreman and Murnighan 1996). In such a setting, winning the auction provides a strong signal that one's estimate of cost or value is too optimistic. In contrast, some of the variation in bids in our setting (as in many natural settings) is due to variations in actual cost. This reduces the information about cost-estimation error conveyed by winning the auction, because the winning bidder may have a true competitive advantage. However, the high variation in cost estimates, relative to variation in actual costs, makes the winner's curse almost as strong as in common-value auctions.

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[4] All cohorts received Set 1 in Figure 1 first. Half of the cohorts in each responsibility condition received Set 2 in Figure 1 in regime 2, followed by Set 3 in regime 3, while the other half received these two sets of initiatives in the reverse order.

The second difference from prior studies is that we do not inform participants of the parameters of the setting; rather, they must learn those parameters from experience. In particular, they must attribute past bids and earnings outcomes to either noise in cost estimates or variation in competitive advantages or both. Because participants cannot know in advance that variation in cost errors is high relative to variation in competitive cost advantage, the winner's curse is quite likely to arise in early periods. Our focus is on how a sense of responsibility for the competitive cost advantage alters participants' ability to learn from feedback and avoid the winner's curse in later periods.

Cost Initiatives

For each regime, each seller has a cost-reduction initiative that is either assigned (in the no-responsibility setting) or selected by them (in the responsibility setting). To make the importance of the initiative more salient and provide a reasonable basis for a decision, sellers were given fictitious data regarding each of the initiatives available for each regime. Specifically, they received statistics on five characteristics: mean cost reduction achieved through each initiative at other firms, similarity between the participant's firm and others that used the initiative, percentage of user firms that judged the initiative as successful, total number of firms that used the initiative, and the difficulty and cost of implementation. Figure 1 presents the complete statistics and explanatory material on initiatives provided to participants.

We made no statements as to the source or validity of the statistics. The statistics were intended to allow sellers in both conditions the opportunity to believe they could assess the likelihood of having a competitive advantage (an assessment they needed to make to determine bidding strategies in both conditions). In the responsibility condition it also allowed sellers the opportunity to believe they had chosen the best initiative because they knew which dimensions of information were most important. The choice or assignment of cost-management initiatives did not change the distribution from which individual sellers' cost reductions were drawn. Participants were told that costs of sellers with different initiatives might or might not differ. In fact, the choice of cost initiative had no effect on actual costs, which were based on predetermined random variables. (Thus, some sellers had a competitive cost advantage over others, but the advantage did not depend on which cost-management initiative they had chosen.) While participants probably expected cost initiatives to have larger and more consistent effects on costs, such beliefs should have been constant across both the responsibility and no-responsibility treatments; thus, deviation of actual from expected parameters could not account for any differences across those treatments.[5]

Bidding and Feedback

In each period, participants learned their own estimated costs and entered bids from $0 to $1,500. The production contract was assigned to the lowest bidder at the price they bid (ties were broken randomly). After each round of trading, participants learned all four bids, whether they personally entered the winning bid, and the earnings of the winning bidder, as shown in Figure 2. By providing participants with vague prior knowledge about the

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[5] According to the background information on cost initiatives shown in Figure 1, the mean proportion of users for whom cost-management initiatives were unsuccessful in the past was 45 percent. Based on this information, sellers should have been prepared for the possibility that their choice of initiative would not make a difference to costs.

FIGURE 1
Information on Past Performance of Cost Management Initiatives

Set 1

	Mean Cost Reduction	Similarity (0–4 index)	% Successful	Total Users	Implement (0–7 index)
A	25%	2.20	41%	42	2.31
B	24%	2.92	55%	22	2.87
C	17%	1.64	73%	37	4.34
D	22%	1.32	62%	27	5.11
E	20%	2.48	33%	49	3.85

Set 2

	Mean Cost Reduction	Similarity (0–4 index)	% Successful	Total Users	Implement (0–7 index)
F	22%	2.60	37%	52	3.64
G	19%	1.72	77%	35	4.55
H	26%	3.08	52%	25	3.01
J	27%	2.08	43%	44	2.59
K	23%	1.48	65%	29	5.39

Set 3

	Mean Cost Reduction	Similarity (0–4 index)	% Successful	Total Users	Implement (0–7 index)
L	18%	1.92	84%	37	4.34
M	25%	3.36	55%	23	3.36
N	28%	2.20	48%	42	2.45
P	21%	2.48	35%	56	3.85
Q	22%	1.40	62%	32	5.88

Participants received the following explanation of the information on past performance of the initiatives:

(1) Mean cost reduction achieved through this initiative at other firms. Other things equal, greater cost reduction is better.

(2) Similarity between your firm and others that have used this initiative, rated on a 1–4 scale. A higher rating means more similarity. Success in cost reduction in a very different industry or different size of firm may not be predictive of cost success for your firm.

(3) Percent of firms at which this initiative was judged as successful. A high mean cost reduction could be driven by one or two firms only, while other firms had no success at all.

(4) Total number of firms who have used this initiative. Even if 100 percent of users of the initiative were successful in reducing costs, this is not very informative if only one or two firms have used the initiative.

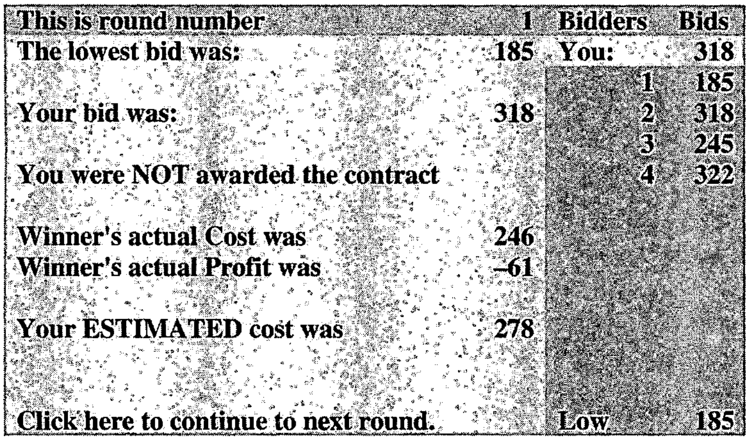
(5) User ratings of difficulty and cost of implementation, rated on a 1–7 scale. A higher rating means an easier and less costly implementation.

distributions of costs and cost estimates, but providing extremely informative feedback, we maximize the extent of learning over the course of the session.

Payments

When all market sessions were completed, laboratory dollars were converted to U.S. dollars and investors received their cash payments. The conversion from laboratory to U.S. currency, with US$ Payment = (Net Earnings in Laboratory $ ÷ Adjustment Factor)

FIGURE 2
Feedback Screen



This is round number	1	Bidders	Bids
The lowest bid was:	185	You:	318
		1	185
Your bid was:	318	2	318
		3	245
You were NOT awarded the contract		4	322
Winner's actual Cost was	246		
Winner's actual Profit was	–61		
Your ESTIMATED cost was	278		
Click here to continue to next round.		Low	185

Participants saw this screen after every round of bidding. The right-most column shows the four bids (as well as repeating the participant's bid in the top row). The line now indicating "You were NOT awarded the contract" would say "You WERE awarded the contract" if the participant was the low bidder in that round.

× Exchange Rate. Participants were not told in advance what the exchange rate or the adjustment factor would be. However, they were told that the exchange rate was positive, meaning that the more laboratory dollars they won, or the fewer they lost, the more U.S.$ they would take home. They were also told that minimum payment was $5 per hour, and that average payments would be approximately $20. Concealing the adjustment factor and exchange rate was intended to avoid risk-seeking behavior among traders who might otherwise know they would receive the minimum payment.

IV. MAIN RESULTS

We structure our analysis by first establishing that sellers in the responsibility condition increase padding more slowly than those in the no-responsibility condition. We then examine the causal links that lead to that outcome by examining responses to feedback and associations between padding, beliefs about cost competitiveness, and beliefs about error in cost estimates. Finally, we rule out alternative explanations for our results.

Analysis of Padding

We hypothesize that sellers can learn to avoid the winner's curse, but that learning will be less effective in the responsibility setting. To test this hypothesis, we examine the extent to which sellers "pad" their bids by bidding above the estimated cost provided by their cost accounting system. Padding, defined as bid minus estimated cost, is our most direct measure of sellers' strategies.[6] (While earnings could also be considered an indicator of sellers' strategies, it is a noisier indicator because, unlike padding, it is also influenced by other variables like realizations of actual costs and other sellers' bids.) In the course of the experiment, mean padding never became so large that a further increase would have been suboptimal (mean earnings never became positive); thus, mean padding increases can be interpreted as positive learning throughout the experiment.

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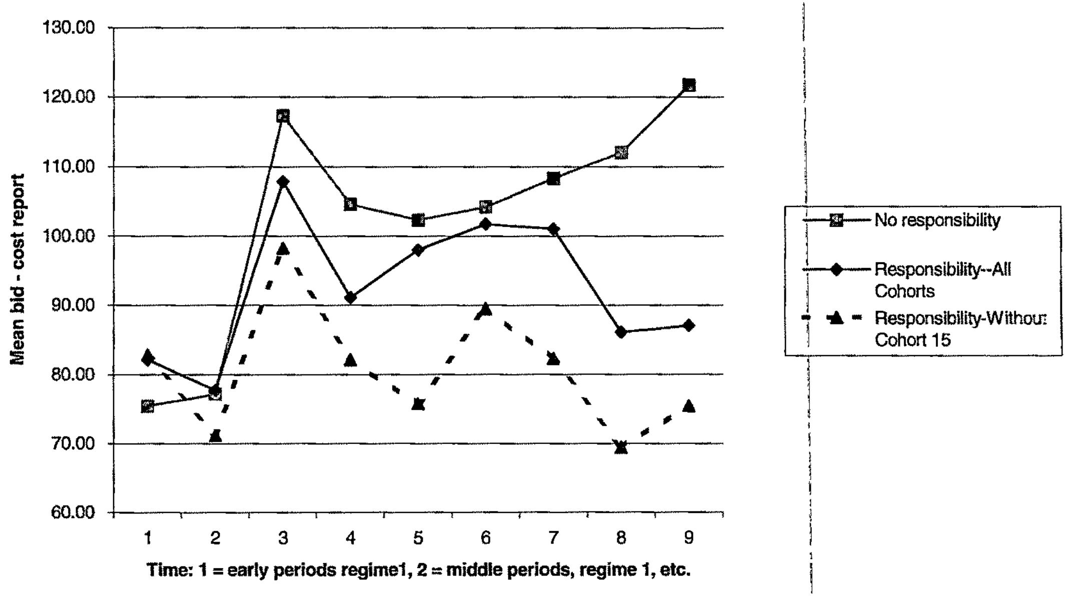
[6] We do not attempt to determine optimal padding. Optimal bids depend on individual conjectures about noise and competitive positions. These conjectures change in response to feedback, and should result in an evolution of padding toward the level that generates non-negative returns.

For our statistical analysis of padding, we divide each regime into three segments: early (periods 1–5), middle (periods 6–9), and late (periods 10–14), and calculate mean padding per period in each segment. Figure 3 and Table 1 show evidence that sellers learn to increase their padding and do so more effectively in the no-responsibility setting. Panel A of Table 1 shows descriptive statistics for all participants. Figure 3 and the tests reported in Panel B of Table 1 show results with and without an outlier cohort in the responsibility condition ("cohort 15"). Inclusion of the outlier tends to distort the condition mean, since the mean padding per period in cohort 15 is 4.7 standard deviations from the mean of the other seven cohorts in the responsibility treatment (173.7 versus 81.1).[7]

To assess statistical significance, we estimate a repeated-measures ANOVA with padding as the dependent variable and regime (1–3), segment (early, middle, late), and responsibility treatment (responsibility, no-responsibility) as independent variables. Our hypothesis predicts an interaction between responsibility and experience. Because learning

FIGURE 3
Padding of Bids to Adjust for Cost-System Error, with and without Responsibility for Cost Management



Each point on the graph represents mean padding per period for the periods within the indicated time segment (e.g., the first five periods of the first regime, the next four periods of the first regime). Means for the responsibility condition are shown with and without the outlier cohort.

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[7] The only three sellers who earned positive profits in the responsibility condition were from this cohort. Moreover, three of the four sellers in this cohort made identical cost-management initiative choices in each regime, whereas in all other cohorts, each of the four sellers chose a different set of cost-management initiatives across the three regimes. We cannot exclude the possibility that sellers in this cohort found some means of colluding.

TABLE 1
Padding of Bids to Adjust for Cost-System Error

Panel A: Descriptives

Mean (s.d.) of padding (= bid − estimated cost) per period, all cohorts. Segments: early = first five periods of a regime, middle = next four periods; late = last five periods.

Regime	Segment	No Responsibility	Responsibility
1	Early	75.4 (37.2)	82.2 (36.2)
1	Middle	77.2 (39.8)	77.8 (31.6)
1	Late	117.3 (22.3)	107.8 (34.3)
2	Early	104.6 (49.3)	91.1 (36.2)
2	Middle	102.3 (46.6)	98.0 (65.5)
2	Late	104.2 (31.9)	101.8 (46.9)
3	Early	108.3 (35.9)	101.0 (55.0)
3	Middle	112.0 (38.2)	86.1 (51.8)
3	Late	121.8 (36.0)	85.1 (40.0)
1, mean		90.0 (24.5)	89.3 (24.8)
2, mean		103.8 (38.5)	97.0 (44.7)
3, mean		114.1 (34.8)	90.7 (47.6)
	Early, mean	96.1 (35.8)	91.4 (35.5)
	Middle, mean	97.2 (39.2)	87.3 (47.3)
	Late, mean	114.4 (24.2)	98.2 (36.9)

Panel B: Hypothesis Tests

F-tests of responsibility × experience interactions in repeated-measures ANOVAs.

	F, all cohorts (n = 16)	F, without cohort 15 (n = 15)
Responsibility × regime	4.23*	11.29**
Responsibility × segment	1.09	4.16*
Responsibility × total experience (early Regime 1 versus late Regime 3)	5.26*	13.50**

*, ** $p < .05$ and $p < .01$, respectively (two-tailed).

effects may be different for different kinds of experience (across segments with the same cost-management initiative and across regimes with different cost-management initiatives), we include regime and segment as distinct independent variables representing experience, rather than simply using period (1–42) as a single independent variable representing experience. We also perform a separate repeated-measures ANOVA to test for an effect of responsibility condition on total learning. In this test, the repeated measures are mean padding in the first and last five periods of the experiment, and the independent variable is responsibility condition. Because observations of individual behavior within a cohort are not independent, we use cohort (n = 16) as the unit of analysis for all ANOVAs. (Thus, each observation is a cohort's mean padding per period in a segment of a regime.) Table 1, Panel B reports the key results of these ANOVAs, with the first two lines reporting on the first ANOVA (regime and segment effects) and the third line reporting on the second ANOVA (total learning).

We find that the change in padding across regimes varies systematically with responsibility condition (regime × responsibility F = 4.23, p = 0.04, all cohorts; F = 11.29, p = .002 without cohort 15). This interaction reflects the increase in average padding from 90.0 in regime 1 to 114.1 in regime 3 in the no-responsibility condition, compared to a negligible change from 89.3 to 90.7 in the responsibility condition (all cohorts).

The effect of segment (early, middle, and late periods within each regime) varies with responsibility condition, although this variation is statistically significant only when the outlier cohort is omitted (F = 1.09, p = .37, all cohorts; F = 4.16, p = .04 without cohort 15). This interaction reflects the increase in padding from 96.1 in early segments to 114.4 in late segments in the no-responsibility condition, compared to a smaller change from 91.4 to 98.2 in the responsibility condition (all cohorts).[8]

Finally, we compare only the early segment of regime 1 with the late segment of regime 3, to test the total effect of experience both across and within regimes. This analysis also shows a significant interaction between experience and responsibility, with an overall increase of padding from 75.4 to 121.8 for the no-responsibility group, compared to an increase from 82.2 to 85.1 for the responsibility group (F = 5.26, p = .04, all cohorts; F = 13.50, p = .003 without cohort 15). These results are robust to different choices of the number of periods to include in the early segment of regime 1 and the late segment of regime 3.[9] Thus, analysis of padding strongly supports our prediction that sellers will be less successful in learning to avoid the winner's curse when they feel responsibility for cost management.[10]

V. SUPPLEMENTARY ANALYSES

We now present more direct evidence that (1) sellers' self-reported beliefs at the end of regime 3 are consistent with the biased attributions we hypothesize, and are associated with actual bidding behavior as predicted, (2) sellers' period-to-period learning from accounting feedback (their own and other profits) differs by responsibility condition, in a manner consistent with these beliefs, and (3) the evidence is inconsistent with plausible alternative explanations.

Identifying Causes of the Responsibility Effect

We analyze debriefing data to provide supporting evidence for the explanation put forward in the hypothesis motivation. This explanation has two essential elements. First, we predict that, compared to bidders without responsibility for cost management, sellers with such responsibility attribute losses disproportionately to errors in cost estimates, rather than to poor competitive position. As a result, we predict that these sellers will perceive they have a greater competitive advantage (or less disadvantage) than sellers without such responsibility, while also perceiving they have greater error in cost estimates. Second, we predict that the net effect of these misperceptions is to reduce bidders' padding, because increases in perceived cost competitiveness will decrease padding more than increases in perceived cost-estimate error will increase it.

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[8] There are no significant ($p < .10$) responsibility × regime × segment interactions in any of these analyses.

[9] We compare the first period versus the last period, the mean of the first two periods versus the mean of the last two periods, and so on. All aggregation choices from one to ten periods show a significant interaction between responsibility and experience ($p < .05$, one-tailed, with or without cohort 15).

[10] Similar analyses using mean earnings rather than padding as the dependent variable show that between the first segment of regime 1 and the last segment of regime 3, losses decrease more rapidly in the no-responsibility condition (from −49.6 to −0.45) than in the responsibility condition (from −67.0 to −40.5) ($p < 0.05$ for responsibility × experience interaction, with or without cohort 15). Comparing *all* segments of regime 3 with all segments of regime 1, the difference is significant ($p < 0.05$) only if cohort 15 is excluded. While these results are less striking than those for padding, the strong influence of random actual-cost realizations on earnings reduces our power to identify treatment effects. Given that such effects are still apparent, we conclude that the market mechanism is unable to keep the psychological bias from affecting market-level prices and earnings. These results contrast with those of Kachelmeier (1996), who showed that sunk-cost biases influenced individual behavior but not prices and profits in a double-auction market, possibly because our one-sided product market imposes less discipline than a double auction.

To test the first element of this explanation, we compare judgments about competitive-cost advantage and cost-estimation error across responsibility and no-responsibility conditions. Specifically, after all market sessions were complete, we asked sellers "Considering only the LAST SET OF 14 ROUNDS, how do you think your actual costs compared with your competitors' costs, on average?" (The response scale indicated dollar estimates of the amount by which their costs were lower or higher than their competitors' costs.) We also asked, "Considering only the LAST SET OF 14 ROUNDS, what was the average [absolute] difference between your costs and your cost estimates?"

Consistent with the basic attribution model, those who assess greater cost-competitiveness also perceive greater cost-estimation errors (Pearson correlation between the two judgments, $r = .26$, $p = .04$).[11] By the end of the experiment, sellers have a negative view of their performance: on average, they believe they had a cost *dis*advantage compared to their competitors. However, sellers with cost-management responsibility believe that this disadvantage is smaller than sellers without responsibility (mean estimates of $12.82 and $20.31, respectively), and that their cost-estimation error is larger (means of 102.50 and 84.69). The effect of responsibility condition on judgments of cost-competitiveness is significant ($p < .05$, one-tailed), as shown by a regression of cost-competitiveness judgments on responsibility condition and cost-estimation-error judgments. Similarly, a significant effect of responsibility condition on judgments of cost-estimation error ($p < .02$, one-tailed), is shown by a regression of cost-estimation-error judgments on responsibility condition and cost-competitiveness judgments.[12]

Finally, to test the net effect of these judgment biases on bidding behavior, we regress individuals' mean regime 3 padding on the two judgments, a dummy variable for responsibility condition, and padding in regime 1 (to control for the individual's general bidding strategy, independent of judgments about the regime 3 environment). As predicted, sellers who believe they have relatively better competitive positions in regime 3 pad their bids significantly less in these periods ($p = .02$), but judgments about cost-estimation error do not have a significant offsetting effect on bid-padding ($p = .32$). Controlling for the two judgments, responsibility condition has no incremental effect on padding ($p = .32$), indicating that cost-management responsibility affects bidding primarily through judgments of cost-competitiveness. Overall, analysis of debriefing data are consistent with our prediction that responsibility for cost management leads to persistently aggressive bidding associated with misperceptions of competitive position, even in light of repeated feedback indicating that less aggressive bids would reduce losses.

Analysis of Learning from Feedback

To provide more direct evidence of how responsibility for cost management affects sellers' period-by-period learning from feedback, we estimate for each group the regression:

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[11] Tests reported in this subsection and the following subsections use data from all cohorts. If tests that exclude cohort 15 yield qualitatively different results, then they are reported in footnotes.

[12] Because of the associations among responsibility condition, judgments about competitive advantage, and judgments about cost-estimation error, any test that includes only two of these variables (e.g., simple comparisons of mean judgments across responsibility conditions) may be biased by a correlated omitted variable. Therefore, when we test for effects of responsibility condition on either judgment, we control for the other judgment.

$$PadChange_{i,t} = a + b_1{*}CohortProfit_{t-1} + b_2{*}WinDum_{i,t-1} + b_3{*}(CohortProfit_{t-1}{*}WinDum_{i,t-1}) + \varepsilon_{i,t},$$

where:

$PadChange_{i,t}$ = change in padding of bidder i from period $t-1$ to t;
$CohortProfit_{t-1}$ = profit of low bidder in cohort in period $t-1$; and
$WinDum_{i,t-1}$ = a dummy variable with value 1 if trader i was the low bidder.

We conduct a pooled regression for each responsibility setting that includes one observation for every seller-period for which we have the necessary data (8 cohorts × 4 sellers/cohort × 13 periods/regime × 3 regimes = 1248; actual n's are smaller because some participants did not submit bids in some periods). Results are shown in Table 2.

The results in Table 2 show that responsibility and no-responsibility sellers react differently to the magnitude of prior-period losses. In both conditions, the more money a winning bidder loses in period $t-1$, the more other sellers increase their padding in period t to protect themselves against similar losses ($b_1 = -0.10$ in no responsibility and -0.19 in responsibility, $p < .001$). However, no-responsibility sellers increase their padding four times as much when the loss in the prior period was their own ($b_1 + b_3 = -.42$) than when it was another seller's ($b_1 = -0.10$; b_1 and b_3 each significant at $p < .001$). Thus, no-responsibility sellers not only improve their bidding strategies in response to losses, but also (rightly) respond more to their own losses than to others' losses, which are less informative about their own costs.

In contrast, responsibility sellers do not increase their padding significantly more after they themselves have lost money than after others have lost (insignificant b_3 coefficient, $p < 0.13$). Compared to winners without cost-management responsibility, they appear less willing to interpret their own losses as strong indications that their own low cost estimates

TABLE 2
Analysis of Learning from Feedback

Effects of previous-period profit on current-period padding for winners and losers in previous-period auction.

$$PadChange_{i,t} = a + b_1{*}CohortProfit_{t-1} + b_2{*}WinDum_{i,t-1} + b_3{*}(CohortProfit_{t-1}{*}WinDum_{i,t-1}) + \varepsilon_{i,t}$$

	Estimates of Coefficients on: (p-value)					
	Intercept (a)	Cohort Profit (b_1)	Winner Dummy (b_2)	*CohortProfit* WinDum* (b_3)	R^2	n
No responsibility	−15.1 (.001)	−0.10 (.001)	41.5 (.001)	−0.32 (.001)	0.086	1236
Responsibility	−21.3 (.001)	−0.19 (.001)	47.5 (.001)	0.13 (.13)	0.088	1082

$PadChange_{i,t}$ = change in padding of bidder i from period t–1 to t;
$CohortProfit_{t-1}$ = profit of low bidder in cohort in period $t-1$; and
$WinDum_{i,t-1}$ = a dummy variable with value 1 if trader i was the low bidder.
Pooled regression, all cohorts.

are unreliable (i.e., that they do not have a significant competitive cost advantage) and their padding should increase.[13]

In order to determine whether the coefficients shown in Table 2 differ significantly across responsibility conditions, we perform a combined regression with dummy variables to capture the main effects of responsibility and its interactions with cohort profit and the winner dummy. Only b_3, the indicator of differential response to own versus others' losses, differs significantly across the two conditions (coefficient on responsibility **Cohort Profit*WinDum* is significant at $p = 0.051$, one-tailed).[14] Overall, our analysis of learning is consistent with our hypothesis that sellers who feel responsibility for their competitive advantage will have difficulty learning from feedback that potentially reflects negatively on that advantage.

Ruling Out Alternative Explanations

Failure to learn in the responsibility condition is not due to lower levels of attention and comprehension in that condition. In fact, after they have chosen a cost-management initiative, sellers in the responsibility condition take significantly longer to choose their bids than those in the no-responsibility condition (means of 20 seconds versus 15 seconds per bid, respectively; $t = 2.83$, $p = .01$), suggesting that sellers with cost-management responsibility pay as much or more attention to the bidding task than those without responsibility. Moreover, participants with cost-management responsibility have at least as accurate an understanding of their market performance as sellers without responsibility. A post-experiment question asked sellers to estimate their relative profit standing (the percentage of sellers in other cohorts with earnings lower than their own). The Pearson correlation between relative-profit estimates and actual profits was significant both for sellers with cost-management responsibility ($r = .66$, $p = .00$) and sellers without responsibility ($r = .46$, $p = .01$).

If most sellers in the no-responsibility condition were assigned to initiatives they did not prefer, the less aggressive bidding in the no-responsibility condition might be explained by their beliefs that the assigned cost-management initiative was unlikely to be successful and actual costs were unlikely to be as low as the lower cost estimates. To exclude this alternative explanation, we test whether sellers' bidding behavior in the no-responsibility condition differs depending on which initiative was assigned to them. If no-responsibility sellers who were assigned the initiatives more favored in the responsibility condition bid more aggressively (more like those in the responsibility condition), then we would conclude that perceived value of the initiatives, rather than responsibility as such, accounts for the difference in behavior across experimental conditions.[15]

The tests for initiative effects are ANOVAs with mean padding by individual in each regime as the dependent variable and initiative assigned as the independent variable. In regime 1, no-responsibility sellers who receive the initiative more commonly preferred in the responsibility condition *do* bid differently (F = 3.34, p = .05), which gives us some confidence that this test can capture the influence of perceived initiative value. In regimes 2 and 3, however, the effect disappears (F = 0.08 and 1.18, p = .97 and .32, respectively). These results are consistent with sellers in the no-responsibility condition learning from experience that, in this setting, the initiatives are not a dominant determinant of profitability. Bidders in the responsibility condition do *not* learn the relative unimportance of initiatives so readily, perhaps because of their involvement in initiative choice. Therefore it appears that the less aggressive bidding in the no-responsibility condition cannot be accounted for by sellers' receiving a less-preferred initiative.

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[13] The negative intercepts and large positive coefficient on *WinDum* indicate that sellers decrease padding after failing to win the bid and increase padding after winning the bid. These changes in padding may reflect mean reversion, if there is a random element in padding and individuals won bids when their padding was unusually low.

[14] Parameter estimates are similar when excluding cohort 15, but the coefficient on b_3 does not reach conventional levels of significance ($p = 0.12$, one-tailed).

[15] Sellers' preferences over initiatives in the responsibility condition were fairly diverse: the modal pattern was for the four sellers in a responsibility cohort to choose three different initiatives. However, sellers displayed some preference for the initiative that had been used at firms most similar to their own. We assume that sellers who were randomly assigned to the no-responsibility condition had preferences similar to those displayed by sellers randomly assigned to the responsibility condition.

Finally, responsibility effects are not driven by differences in sellers' perceptions of bias in their cost estimates. We asked sellers, after the completion of all market sessions, "Considering only the last set of 14 rounds, what was the average bias in your estimated costs?" Sellers in the no-responsibility setting believed estimated costs were negligibly below actual costs (mean = −3.75, std. dev. = 74.0), while sellers in the responsibility setting believed that estimated costs were negligibly above actual costs (mean = 0.63, std. dev. = 68.1). The means are not significantly different from one another, nor are they significantly different from zero ($p > 0.3$ for all comparisons). Thus, on average, sellers in both conditions believed that their costs were unbiased: in both conditions they had the same understanding of the pricing task as adjustment for errors that could either under- or overestimate product costs.

V. DISCUSSION

Prior research has shown that individuals can learn from market feedback to adjust appropriately for erroneous or irrelevant information included in cost estimates (Briers et al. 1999; Gupta and King 1997; Kachelmeier 1996; Waller et al. 1999). In these prior studies, individuals make only one type of decision, usually pricing. Our study contrasts the performance of individuals with responsibility for only pricing decisions and the performance of individuals with responsibility for cost-management decisions as well as pricing. In a setting where cost-estimation error initially induces a severe winner's curse, market feedback eliminates the curse for sellers who make only pricing decisions, but fails to do so for sellers who make both cost-management and pricing decisions.

Although sellers with cost-management responsibility spend longer on the pricing decisions than do sellers with only pricing responsibility, they are less successful in improving their pricing decisions. They overestimate their competitive cost advantage (or underestimate their disadvantage), compared to sellers with pricing responsibility only; in consequence, they add smaller "pads" to their cost estimates when bidding. When they lose money on a winning bid because actual costs are higher than estimated, sellers in both experimental conditions pad their next bid more; but this learning effect is smaller for sellers with cost-management responsibility than for those without. We suggest that this behavior is consistent with different causal attributions for market outcomes in the two conditions. Sellers can attribute money-losing bids either to the fact that their actual costs tend to be high or to the fact that they have had "bad luck" with inaccurate cost estimates. Giving sellers cost-management responsibilities appears to decrease their propensity to make the former attribution, which in turn leads them to bid more aggressively.

While our experimental setting is quite simple, it is substantially more complex than most prior studies of the winner's curse. As in real product markets, sellers have imperfect knowledge of the probability distributions of their competitive position, their production costs, and their cost-estimation errors; they must learn about these distributions from experience. We show that even in this challenging environment, sellers can learn to avoid the winner's curse, but they are severely hampered by a combination of job responsibilities that induces biased interpretations of market feedback. The deleterious effects of this bias are likely to depend on the variation in sellers' costs, relative to their cost-estimation errors. We focus on a setting in which variation in cost is low relative to cost-estimation errors. In such a setting, the winner's curse is quite severe, and it is relatively easy for sellers to draw incorrect inferences about their competitive position. If sellers' costs varied widely and cost-estimation errors were small, then the winner's curse would not be particularly severe even in early rounds, and it would be more difficult for sellers to believe they have a competitive advantage when they actually have a substantial disadvantage.

The effects we observe are related to escalation bias, in which people suboptimally continue or increase commitment to a previously chosen course of action that has had poor results (Staw and Ross 1989; Brockner 1990). More aggressive bidding in the responsibility condition could be viewed as a form of escalation, in that it is a continuing costly commitment to the seller's belief that he or she has chosen the most effective cost-management initiative. Such aggressive bidding in the face of feedback requires sellers to misinterpret feedback to make the escalation decisions seem appropriate. Few studies on escalation have looked at escalation decisions that are as indirect as ours (a typical study would focus on individuals' willingness to reverse or continue a prior decision like the cost-management initiative choice itself, rather than the subsequent bidding decisions), and few have examined responses to feedback in a noisy environment that would allow attribution biases to distort beliefs and decisions as severely as does ours. Thus, a contribution of our paper is to show that escalation effects can be more indirect and far-reaching than those typically discussed, and that attribution biases can be a mechanism for escalation behavior in the face of feedback.[16]

Our results also point out important limitations to the common argument that accounting-related decision errors will be eliminated through market discipline. Reduction of the winner's curse through market discipline is limited in our setting, as in most product markets, because an unbiased seller has no opportunity for arbitrage by buying an underpriced good from a competitor; as a result, marginal prices are set by sellers who underbid the most.[17] As a result, discipline must occur either through the bankruptcy of low bidders or through bidders' responses to feedback. Although underpricing of a particular good or service can cause economically relevant reductions in profit, it will rarely be sufficient to cause rapid bankruptcy among multiproduct companies in environments beset by many random influences. As a result, learning from feedback, rather than bankruptcy, is likely to be the primary source of pricing-decision improvement, especially in the short term. Even though our feedback is designed to be more informative than would be expected in most product markets (all sellers in our market observe the exact profit or loss of the low bidder), self-serving biases interfere with learning and allow pricing errors to persist.

Our examination of job-design effects has some elements in common with principal-agent models that examine the interrelations of job design, incentives, and information, identifying which tasks are best bundled or separated in order to support effective incentive contracting. Most agency models include only conscious, optimizing responses to job design

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[16] Motivated reasoning could also drive beliefs that support escalation (Kunda 1990). We thank an anonymous reviewer for pointing out the relation of our work to escalation theory.

[17] Ganguly et al. (1994) show how short-selling restrictions can cause similar problems in financial markets.

(e.g., Hemmer 1995; Nikias et al. 2005). The unconscious influences of job design on behavior that we identify in our experiment are more similar to the social-norm effects examined by Fischer and Huddart (2004). Similar modeling techniques might be able to formalize the effects we observe in our experiment.

While the learning problems created by joint responsibility are clear, the appropriate solutions are not. Separation of responsibilities improves learning from market feedback but can limit opportunities to learn about actual costs from familiarity with the production process. Separation of responsibilities may also lead bidders and cost managers to blame one another for poor outcomes. Future research might examine how such finger-pointing affects learning from market feedback. Future research might also examine the effects of attribution biases when cost-estimation errors are systematic (perhaps varying with product volume and complexity, as would occur when a firm should, but does not, use activity-based costing), and when the decision-maker has responsibility for cost-estimation errors by selecting the cost accounting system.

Attribution biases might also play a role in the poor post-acquisition performance of acquisitions among "serial acquirers." The individuals responsible for the decision to acquire are typically also responsible for managing the acquired business after the acquisition. They are therefore more likely to ascribe failed acquisitions to bad luck that is out of their control, rather than to overbidding or poor post-acquisition management. Attribution biases might explain why pure accumulation of experience does not increase the success of acquisitions by serial acquirers, although codified knowledge in manuals and systems (perhaps less biased than the private judgment of the individuals responsible for the previous acquisitions) tends to increase success (Leshchinskii and Zollo 2004).

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THE ACCOUNTING REVIEW
Vol. 81, No. 1
2006
pp. 49–82

After Enron: Auditor Conservatism and Ex-Andersen Clients

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ABSTRACT: This study examines whether after Arthur Andersen's demise, successor auditors required more conservative accounting for their ex-Andersen clients in order to minimize litigation risk. We use unadjusted and performance-adjusted measures of abnormal accruals, and we examine the level of and changes in the abnormal accruals of ex-Andersen clients in 2002 relative to a control sample of clients that were audited by a Big 4 auditor in 2001 and 2002. We conduct univariate and multivariate tests. In our multivariate tests, we control for other factors that may affect litigation risk cross-sectionally. Our results indicate that the ex-Andersen clients had lower levels of and larger decreases in abnormal accruals in 2002. This is consistent with auditor conservatism and suggests the successor auditors viewed an Andersen audit as a unique source of litigation risk.

Keywords: *auditor conservatism; litigation risk; abnormal accruals; Arthur Andersen.*

Data Availability: *Data are publicly available from the sources identified in the text.*

JEL Classification: *M41.*

I. INTRODUCTION

We examine whether Big 4 auditors required more conservative accounting for clients they acquired from Arthur Andersen. Recently, Chaney and Philipich (2002) found negative abnormal returns for clients of Andersen around four

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We thank Paul Chaney, David Emanuel, Debra Jeter, Robert Knechel, Donald Stokes, Patricia Dechow (the editor), two anonymous reviewers, and workshop participants at the University of Auckland, University of Melbourne, 2004 AAA Annual Meeting, and 2004 AFAANZ Annual Conference for their useful comments. We also thank Jerry Sun for providing research assistance.

Editor's note: This paper was accepted by Patricia Dechow, Editor.

Submitted March 2004
Accepted August 2005

Enron-related events.[1] One interpretation is that these events impaired the assurance value of Andersen audits. Specifically, as a result of Andersen's involvement with Enron, the financial statements of Andersen's other clients were viewed as being less credible and having a higher probability of misstatements.

Potential misstatements can affect the incoming auditor's litigation risk if the errors are not found and corrected or if overly aggressive accounting policies are allowed to continue under the new auditor (Lys and Watts 1994). DeFond and Subramanyam (1998) suggest that litigation risk can motivate auditors to prefer income-reducing accounting choices. They refer to this preference as auditor conservatism and they show that abnormal accruals are lower when litigation risk is high. Further, Heninger (2001) provides evidence that auditors are more likely to be sued when their clients have abnormal accruals that are income-increasing. Thus, we expect that Big 4 auditors would have incentives to require more income-reducing abnormal accruals for their ex-Andersen clients, particularly in 2002 when concerns about Andersen audits—and the associated litigation risk—were at a peak.

We examine the abnormal accruals of ex-Andersen clients using unadjusted and performance-adjusted measures from the Jones (1991) model. Our performance-adjusted measure is based on Francis et al. (2005). Specifically, we adjust each abnormal accrual by subtracting the median abnormal accrual for the client's industry-ROA decile. We also use negative special items as a third measure to provide a non-Jones model measure of accounting discretion.

We conduct two types of tests. First, we examine the level of abnormal accruals by comparing the 2002 abnormal accruals of the ex-Andersen clients with the 2002 abnormal accruals of a control sample of ongoing Big 4 clients (i.e., audited by one of the Big 4 in 2001 and 2002). Second, we examine the change in abnormal accruals from 2001 to 2002 by comparing the change for the ex-Andersen clients with the change for the ongoing Big 4 clients. We examine the levels and changes in univariate and multivariate tests. Because other factors can affect the litigation risk between clients (e.g., Stice 1991; Lys and Watts 1994), in our multivariate tests we control for 11 factors that are likely to affect the auditor's pre-audit assessment of litigation risk. Thus, our multivariate tests examine whether being audited by Andersen was seen as a significant source of litigation risk by successor auditors after controlling for other litigation risk differences between sample firms.

We find evidence that successor auditors did constrain the abnormal accruals of their ex-Andersen clients in 2002. In our univariate tests, we find significantly lower unadjusted and adjusted abnormal accruals and more negative changes in unadjusted abnormal accruals for the ex-Andersen clients. In our multivariate tests, we find that after controlling for our 11 specific litigation risk factors, the level of and changes in abnormal accruals are lower for the ex-Andersen clients. These findings hold for both our unadjusted and adjusted abnormal accrual measures. This suggests that auditors viewed an Andersen audit as a source of litigation risk. However, our results are strongest for Ernst & Young, which suggests differences in conservatism between Big 4 auditors.

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[1] The four dates are: (1) November 8, 2001 when Congress subpoenaed Andersen to provide Enron-related documents (also Enron announced it would further restate its earnings and balance sheet figures on that day), (2) December 12, 2001 when Andersen's CEO, Joe Bernardino, testified in a Congressional hearing that Andersen made incorrect judgments in auditing Enron, (3) January 10, 2002 when Andersen informed the SEC and Department of Justice that Andersen auditors had shredded documents related to Enron, and (4) February 4, 2002, which was the first trading day after the release of a report that was critical of Andersen (the Powers report that was commissioned by Enron's board of directors) and after the announcement by Andersen that the former Federal Reserve Board Chairman, Paul Volcker, would head up an Independent Oversight Board to investigate Andersen's audit policies and procedures.

Our research contributes to the literature in several ways. First, we contribute to the research on auditor conservatism and litigation risk. For example, prior studies examine litigation risk at a broad level—e.g., Francis and Krishnan (1999) and Kim et al. (2003) use the Big 6/non-Big 6 dichotomy to proxy for litigation risk and Seetharaman et al. (2002) and Khurana and Raman (2004) focus on litigation risk at a country level. Instead, we examine whether the audits conducted by a particular auditor—Arthur Andersen—are associated with greater litigation risk.

Second, our study complements DeFond and Subramanyam (1998) who examine auditor conservatism around voluntary auditor switches. While they find that auditor conservatism leads to lower abnormal accruals prior to the switch, they are unable to completely rule out self-selection bias as a confounding factor. In our study, because the auditor switches made by ex-Andersen clients were caused by an exogenous event (Andersen's loss of reputation and demise), we avoid problems related to self-selection bias and are able to examine the abnormal accruals around the switch more cleanly.[2]

Third, our study contributes to the research that examines the association between accruals (or abnormal accruals) and litigation. Studies like Lys and Watts (1994) and Heninger (2001) treat the level of accruals as exogenous, i.e., they ignore the auditor's role in influencing the level of accruals. Our study complements this research by examining the role of auditors in constraining accruals when litigation risk is high.

Fourth, we contribute to the emerging research on Andersen and Enron (e.g., Chaney and Philipich 2002; Eisenberg and Macey 2004; Krishnan 2005). Arguably, the demise of Andersen is one of the most significant events that has affected the accounting profession in the U.S. At the time of its demise, Andersen was the fifth largest accounting firm with worldwide revenues of $9.3 billion and U.S. revenues of $4.3 billion; it operated in 84 countries and had 77,000 employees (Hawkins and Cohen 2003). In the U.S., it had over 2,300 public companies as clients (Alexander et al. 2002d). Thus, given its sheer scale, any research that sheds light on the causes or effects of Andersen's demise can be interesting in its own right.[3]

The remainder of the paper is divided as follows. Section II provides additional theoretical background. Section III discusses the method and provides the results. Section IV concludes.

II. BACKGROUND AND HYPOTHESIS

Auditor conservatism can be defined as the "auditor's preference for income-decreasing accounting choices" (Kim et al. 2003, 327). DeFond and Subramanyam (1998) argue that this preference is related to the client's litigation risk, and Thoman (1996) develops a model that shows that audit quality is linked to potential litigation damages. Therefore, auditors have incentives to require more conservative accounting to avoid future litigation and possible damages resulting from the litigation. Further, because the litigation risk associated with clients differs cross-sectionally, the degree of conservatism that auditors apply to clients will vary between clients (e.g., DeFond and Subramanyam 1998).

Generally, litigation against auditors arises from situations where investors rely on financial statements and subsequently incur losses as a result of an alleged audit failure

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[2] The auditor changes after the bankruptcy of Laventhol and Horwath were also unique in this regard. Menon and Williams (1994) and Baber et al. (1995) provide evidence on switches by ex-Laventhol and Horwath clients.

[3] On May 31, 2005, the U.S. Supreme Court, in a unanimous decision, overruled Andersen's conviction. The Court ruled that Andersen had not obstructed justice and that a U.S. District Court judge had given the jury hearing the case the wrong instructions. However, one former federal prosecutor said that the decision was more of "a symbolic setback" for the government (e.g., see Savage 2005).

(Stice 1991). In the case of an auditor switch, while the successor auditor is typically protected from litigation resulting from financial statements audited by the previous auditor, the successor auditor's exposure to litigation arises (1) from not correcting prior misstatements and (2) from allowing questionable accounting choices to continue once they have taken over the engagement (Lys and Watts 1994).

One area that auditors are likely to scrutinize is the level of accruals, and prior research shows accruals are related to the incidence of litigation. For example, Lys and Watts (1994) find that auditors are more likely to be sued when total accruals are income-increasing. Similarly, Heninger (2001) finds that auditors of firms with higher abnormal accruals are more likely to be sued. This suggests that accruals can lead to misstatements and that investors believe that the auditor is responsible for ensuring that accruals are not used opportunistically.

However, while studies like Lys and Watts (1994) and Heninger (2001) treat the level of accruals or abnormal accruals as exogenous, the literature on auditor conservatism sees accrual choices (and accounting choices more generally) as being jointly affected by the client's and auditor's preferences. From the auditor's perspective, accruals are important because they affect a client's inherent risk.[4] Francis and Krishnan (1999) note that inherent risk increases because accruals introduce estimation error into the accounting numbers in two ways, i.e., accrual-based earnings involve estimates about future events and managers have incentives to use accruals to portray the firm's results more favorably. The auditor can respond to this uncertainty by requiring accruals that are less income-increasing. That is, because auditors determine what is a reasonable interpretation of GAAP (Francis et al. 1999), they can limit accruals by applying more stringent thresholds in deciding whether accruals are acceptable. Since the penalties for overstated earnings are more severe when litigation risk is high, auditors have incentives to require more conservative accruals when litigation risk is high (DeFond and Subramanyam 1998).[5]

Several studies examine the relation between litigation risk and abnormal accruals. Kim et al. (2003) argue that Big 6 auditors, in particular, have more incentive to constrain income-increasing abnormal accruals because they face higher potential litigation costs. They cite two specific sources for the higher litigation costs. First, Big 6 firms are seen as having "deep pockets." Second, Big 6 firms attract more media attention so litigation is more likely to damage the reputation of Big 6 firms. Kim et al. (2003) find evidence that clients with Big 6 auditors have more income-reducing abnormal accruals than do clients with non-Big 6 auditors.[6]

DeFond and Subramanyam (1998) examine abnormal accruals around auditor switches. They argue that litigation risk motivates the predecessor auditor to prefer conservative accounting choices and, as a result, clients switch to a successor auditor who is less conservative. Consistent with their expectations, they find that the abnormal accruals in the year before the switch are most negative where litigation risk is high. However, one problem with DeFond and Subramanyam's (1998) analysis is that clients self-select into the switching group. Because switching firms in their sample have poorer performance, they are not able to rule out the possibility that their results are driven by financial distress rather than auditor conservatism.

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[4] Inherent risk is the risk that the financial statements are materially misstated and that these misstatements will not be detected by internal controls (SAS No. 47).

[5] Since we view accounting choices as reflecting both the client's and auditor's preferences, our tests do not differentiate between client-initiated and auditor-initiated choices. For example, in some cases, the client may prefer an accounting choice that is too aggressive, but the auditor will object and require a more conservative choice. In other cases, the client may prefer a conservative choice, and given that the client's and auditor's preferences coincide, the auditor has no incentive to change the client's choice. In either case, the outcome reflects the auditor's conservatism.

[6] Francis et al. (1999, 19) note that some "ways in which Big 6 auditors are more likely to constrain aggressive or questionable accounting accruals include better technologies for detecting problem areas, interpretations of GAAP that reduce the scope of aggressive accruals-based earnings management, and strong negotiating stance with clients in terms of required adjustments to the financial statements."

Lee and Mande (2003) also provide evidence that litigation risk can affect the abnormal accruals of an auditor's clients. They examine the level of abnormal accruals of clients before and after the Private Securities Litigation Reform Act was passed in 1995. This act replaced joint and several liability with proportionate liability, which reduced auditors' litigation risk. This benefited the Big 6 auditors in particular, and Lee and Mande (2003) find an increase in income-increasing abnormal accruals for Big 6 clients after the law was passed.

Combined, these studies suggest that (1) litigation risk motivates auditors to prefer more conservative accounting choices, (2) large auditors (e.g., Big 6 auditors) are more sensitive to litigation risk, and (3) auditors are concerned about accruals because they introduce estimation error that can increase litigation risk. In this study, we examine auditor conservatism by investigating whether Big 4 successor auditors require more income-reducing accruals for ex-Andersen clients.

Ceteris paribus, we expect that ex-Andersen clients' successor auditors would only require more conservative accruals for their ex-Andersen clients in 2002 if they believed that the litigation risk associated with an Andersen audit increased as a result of Andersen's involvement in the Enron audit and/or Andersen's indictment and conviction. Prior research suggests that the Enron-Andersen affair did raise general concerns about the quality of Andersen's audits. For example, Chaney and Philipich (2002) find negative and significant returns for a sample of 284 Andersen clients around their January 10, 2002 and February 4, 2002 event dates. This suggests that users had concerns about the credibility of the financial statements previously audited by Andersen, i.e., these events impaired the assurance value of Andersen's audits.

Further, there is ample anecdotal evidence regarding concerns about Andersen's audit quality. For example, shortcomings of Andersen's Enron audit were well documented in the public arena, e.g., in the Powers report prepared by the board of Enron, in Congressional testimony (e.g., see Toffler 2003, 157–158), in the media (e.g., Alexander et al. 2002a, 2002b, 2002c, 2002d), and legal documents (see U.S. District Court 2004). Also, Andersen had been involved in a series of high-profile audit failures including Waste Management, Sunbeam, and the Baptist Foundation of Arizona (see Brickey [2004] for a fuller account), and of the biggest settlements paid by auditors since 1997—the second to sixth largest were paid by Andersen (Hawkins and Cohen 2003).

Lys and Watts (1994) point out that litigation against auditors depends on three factors: existence of a cause, discovery of a cause, and the benefits of litigation (i.e., the size of the damages awarded). They further argue that the existence of a cause is related to the possibility of misstatements, the possibility of audit failure, and the existence of a loss for the plaintiff. The Andersen-Enron affair could have increased litigation risk for the successor auditor in at least two ways. First, if the real audit quality of Andersen was low (e.g., if Andersen's audit procedures are systematically weaker than the Big 4), then the potential for misstatements in the financial statements of former clients would increase, i.e., increase the probability of the existence of a cause. Second, if the perceived audit quality of Andersen is low (e.g., if investors revise downward their beliefs about Andersen's ability to detect and report misstatements), then this could lead investors to scrutinize the financial statements of former Andersen clients more closely. This could increase the likelihood that deficiencies in the financial statements are detected, i.e., increase the probability of the discovery of a cause.

To avoid future litigation, in the case of real problems with audit quality, the incoming Big 4 auditor would have strong incentives to find and correct any misstatements contained in the prior years' Andersen audited financial statements, particularly in the initial audit in 2002. This could involve reversing prior accruals that were too large. In the case of perceived problems, successor auditors can still have incentives to curtail overly aggressive accounting choices because they know that their ex-Andersen clients will be scrutinized more closely. This could involve requiring smaller accruals for events and transactions occurring in 2002, e.g., by using tighter thresholds in deciding what is an acceptable accrual. Consequently, we hypothesize:

> **H1:** Abnormal accruals in 2002 are lower for ex-Andersen clients than for ongoing clients of the Big 4 firms.[7]

Additionally, Chaney and Philipich (2002) find evidence that cumulative abnormal returns (CARs) measured around January 10, 2002 and February 4, 2002 are significantly lower for clients of Andersen's Houston office (which conducted the Enron audit). Also, Krishnan (2005) finds that compared to Houston clients of the Big 4, Andersen's Houston clients reported bad news on a less timely basis, suggesting more aggressive accounting practices. Further, Waste Management, which was accused by the SEC for overstating profits by $1.7 billion in the 1990s, was also a client of Andersen's Houston office and at both Enron and Waste Management, many senior accounting and finance managers were former Andersen employees (e.g., Alexander 2002b, 2002c).[8] This suggests that successor auditors could have perceived systematic problems with Andersen's Houston office. Thus, we also conduct tests that examine whether successor auditors were incrementally more conservative with Andersen's Houston clients.

To examine H1, we use univariate and multivariate tests. Our multivariate tests recognize that other factors can affect litigation risk and can affect auditor conservatism and accruals. Thus, to test whether an Andersen audit is a unique source of litigation risk that affects auditor conservatism, we need to control for these other litigation risk factors.

We include both general litigation risk factors and litigation risk factors that are motivated by specific Enron characteristics. The latter group is included because Doogar et al. (2003) find spillover effects around some Andersen-Enron-related event dates, i.e., they find negative and significant CARs for both Andersen and non-Andersen clients. They suggest that the Andersen-Enron affair led to a period of investor uncertainty and raised general concerns about financial statement quality. Thus, we include Enron-motivated variables to capture the changes in the litigation environment in 2002 that may have affected both Andersen and non-Andersen clients.

We use 11 litigation risk factors and we classify these into three categories, i.e., net benefits of legal action, clients' incentives to manage earnings, and concerns about audit quality. Using Lys and Watts' (1994) framework, the first group roughly corresponds with their existence of loss and net benefit categories, the second group corresponds with their misleading financial statements category, and the third group corresponds with their audit failure category.

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[7] The enactment of the Sarbanes-Oxley Act of 2002 could have also increased litigation risk, but it affected both ex-Andersen and ongoing Big 4 clients. Since we use the latter firms as a control group, Sarbanes-Oxley would affect our tests only if the provisions of the law had a more negative impact on the ex-Andersen firms. Further, Li et al. (2004) find no evidence that their measure for upward earnings management is related to returns measured around key Sarbanes-Oxley event dates. This suggests that the market did not see Sarbanes-Oxley as differentially affecting those firms with incentives to use income-increasing discretionary accruals. Thus, in the context of our study, any differences in abnormal accruals between the ex-Andersen and ongoing Big 4 clients is more likely to be due to the influence of the successor auditor rather than Sarbanes-Oxley.

[8] Andersen paid $95 million to settle lawsuits related to Waste Management (Hawkins and Cohen 2003).

Expected Net Benefits of Legal Action

Generally, litigation risk will be related to the net benefits that are likely to accrue to the plaintiff if legal action is pursued. The expected net benefits will be high when the expected damages are large or when the plaintiff has a high probability of success. We use four measures to capture the expected net benefits associated with legal action.

Client Firm Size

Stice (1991) and Lys and Watts (1994) show that auditors of large clients are more likely to be sued. The size of the client affects the size of damages awarded, and large damages make it more likely that the benefit will exceed the cost of pursuing legal actions. We use the log of total assets to measure the client's firm size.

Abnormal Market Returns

Stice (1991) and Lys and Watts (1994) find that market-based measures are associated with the incidence of lawsuits.[9] We use abnormal stock returns to proxy for the probability that a lawsuit might be filed. Where there are more concerns about financial statement quality, the probability of a lawsuit is higher. Based on Chaney and Philipich (2002), we use the abnormal returns around January 10, 2002 (when Andersen admitted shredding Enron-related documents) and February 4, 2002 (after the Powers report was issued by the board of Enron and after an Independent Oversight Board was established to investigate the problems at Andersen) to measure investors' concerns.

While Chaney and Philipich (2002) find significant negative CARs for ex-Andersen clients around these dates, Doogar et al. (2003) find evidence of a spillover effect, i.e., they find that clients of Big 4 firms also experienced negative CARs, which suggests that investors' concerns about financial statement quality were not confined to Andersen clients. We use the combined CARs from January 10 and February 4 as a proxy for the probability of a lawsuit.

Incidence of Shareholder Litigation

Historically, some industries have been targeted by litigation more than other industries. Francis et al. (1994) identify the biotechnology industry (SIC codes 2833–2836, 8731–8734), computers (SIC codes 3570–3577, 7370–7374), electronics (SIC codes 3600–3674), and retailing (SIC codes 5200–5961) as industries that have a high incidence of shareholder litigation. A high incidence of litigation suggests that plaintiffs believe that the potential damage awards are large and/or the probability of success is high in these industries. Like Frankel et al. (2002), we use an indicator variable based on Francis et al. (1994) to represent high litigation risk industries.

Financial Condition

Palmrose (1988) finds that the majority of lawsuits against auditors involve bankruptcies. Stice (1991) finds that the firm's financial condition, measured using Altman's Z-score

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[9] Stice (1991) finds that the variance of abnormal returns for clients that are sued is higher than for those that are not sued. Lys and Watts (1994) include the rate of return of the client's stock over the prior year, but this variable is not significant in their multivariate test.

(Altman 1968), is negatively related to the likelihood of a lawsuit. We use Altman's Z to measure financial condition where lower values represent weaker financial condition and more litigation risk.[10]

Client's Incentives to Manage Earnings

Kim et al. (2003) expect that auditor conservatism will be most evident where managers have incentives to use income-increasing accruals because the auditor and managers have conflicting incentives. Anderson et al. (2004) provide experimental evidence that indicates that auditors are more skeptical of managers' explanations when their incentives to manage earnings are high. Based on the prior literature, we identify four factors related to managers' incentives to engage in upward earnings management. In each case, greater incentive to manage earnings upward is associated with greater litigation risk since the probability of an overstatement increases.

Leverage

There is widespread evidence that firms that are close to violating debt covenants use income-increasing accruals (e.g., DeFond and Jiambalvo 1994; Dichev and Skinner 2002). Duke and Hunt (1990) show that highly levered firms are closer to violating their debt covenants. We use the ratio of total liabilities to total assets to measure leverage.

Growth

Growth firms have incentives to meet earnings forecasts because markets react strongly if they do not (Skinner and Sloan 2002). Matsumoto (2002) finds evidence that managers use positive abnormal accruals to avoid negative earnings surprises. Also, managers in growth firms tend to have more discretion in terms of accounting choices (Smith and Watts 1992), and internal controls may be weaker in growth firms (Hall and Renner 1988). Similar to Chung and Kallapur (2003), we use the market-to-book ratio to measure growth.

New Equity Issues

Rangan (1998), Teoh et al. (1998), and others provide evidence that managers have incentives to increase earnings before seasoned equity offers. Kim et al. (2003) find that for their sample of firms with income-increasing incentives, share increases are associated with larger and more positive abnormal accruals. Based on that study, we use an indicator variable to identify firms where the number of outstanding shares increased by more than 10 percent in 2002.

Implicit Claims

Bowen et al. (1995) find that firms that rely on implicit claims with their stakeholders such as customers, suppliers, and employees have incentives to manage earnings upward. These implicit contracts are self-enforcing in that the firm's reputation will be damaged if the implied commitment is not met. Since these stakeholders use financial reports to assess the firm's reputation, Bowen et al. (1995) argue that firms with higher implicit claims have stronger incentive to use income-increasing accounting methods in the long run.

Matsumoto (2002) finds that implicit claims are positively related to the frequency of positive abnormal accruals. Following Matsumoto, we create a measure of implicit claims by factor analyzing three measures related to the production of durable goods, R&D spending, and labor intensity.[11]

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[10] Interpreting the causality of Altman's Z can be problematic. While we expect that auditors will force write-downs and lower accruals when bankruptcy risk is high, instead lower accruals could be the result of poor performance. For example, Butler et al. (2004) find that distressed firms have large negative accruals.

Concerns about Audit Quality

Lys and Watts (1994) note that audit failure can create a cause for legal action. Further, changing perceptions about audit quality can affect the likelihood that a cause will be discovered since investors who become more concerned about audit quality will scrutinize the financial statements more closely. The Andersen-Enron affair led to a period of growing concerns about audit quality, not only for Andersen, but also for other auditors as well. For example, Doogar et al. (2003) find evidence of spillover effects around Andersen-Enron event dates, and, more obviously, the Sarbanes-Oxley Act of 2002 included major provisions aimed at improving audit quality.[12] This suggests that litigation risks related to audit quality were increasing in 2002 for many firms regardless of whether they were former Andersen clients. To recognize these changes, we use three measures that are related to specific Enron characteristics.[13]

Sales Growth

Chaney and Philipich (2002) find that ex-Andersen clients with higher sales growth had lower CARs measured around January 10, 2002 and February 4, 2002. They suggest that the market may have downgraded these firms for using overly aggressive revenue recognition methods. More generally, SAS No. 99, which was approved in October 2002, identifies revenue recognition as a major source of fraud risk and requires specific analytical procedures related to revenue recognition (SAS No. 99, paragraph 29). Thus, high sales growth could be associated with lower audit quality and higher litigation risk for all firms. Similar to Chaney and Philipich (2002), we use a one-year percentage change in sales to measure sales growth.

Member of Oil and Gas or Electric Utility Industries

Chaney and Philipich (2002) also find evidence that CARs for ex-Andersen clients are negatively related to membership in the oil and gas and electric utilities industries, two industries in which Enron operated. Thus, being an ex-Andersen client in one of these industries could signal a higher probability of financial statement errors and higher litigation risk. However, it is also possible that investors saw Enron's problems as being symptomatic of wider, fundamental issues in those industries. For example, in 2002, the SEC investigated Reliant Resources Inc. (which was audited by Deloitte & Touche) for increasing profits using round-trip trading or power swaps, and the Federal Energy Regulatory Commission ordered 143 energy companies to disclose whether they were involved in such transactions (e.g., see SEC, Reliant settle ... 2003). This could have raised litigation risk for oil and gas and electric utility firms generally. We use an indicator variable to identify clients that are members of the oil and gas or electric utility industries.

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[11] Durable goods is included to represent implicit claims with customers who have to rely on the firm for continuing support and service over the life of the product. R&D is included to represent the uniqueness of a product since unique products must be serviced by the firm that sells the product. Also, R&D creates implicit claims with employees because their skills are more likely to be firm- and job-specific. Labor intensity is another measure of implicit claims with employees.

[12] For example, the Sarbanes-Oxley Act establishes an independent board to oversee audits of public companies (the Public Company Accounting Oversight Board), bans nine types of nonaudit services, requires mandatory rotation of audit partners every five years, prohibits the auditor from becoming the CEO, Controller, CFO, or Chief Accounting Officer for a client firm within a one-year period of completing an audit, and requires the auditor to report "critical" accounting policies to the audit committee.

[13] The cumulative abnormal returns measured around January 10, 2002 and February 4, 2002 can also be viewed as being motivated by Enron, but since we use it as a proxy for the probability of a lawsuit, we include it in the net benefits category.

Ratio of Nonaudit to Audit Fees

Lys and Watts (1994) find evidence that where auditor independence is low (i.e., where the ratio of client sales to total sales audited is high), auditors are more likely to be sued. Numerous studies including Frankel et al. (2002) and Chung and Kallapur (2003) examine whether the provision of nonaudit services impairs auditors' independence. Only Frankel et al. (2002) find evidence that nonaudit fees are associated with earnings management. However, concerns about the level of nonaudit fees at Enron were widely voiced (e.g., see Chaney and Philipich 2002, 1223), and the Sarbanes-Oxley Act of 2002 contains specific provisions aimed at nonaudit fees (e.g., it bans nine specific types of nonaudit services). Thus, higher nonaudit fees could be associated with lower auditor independence and lower audit quality on a general basis. We use the ratio of nonaudit to audit fees to proxy for auditor independence.

To sum up, in our multivariate analysis, we control for 11 factors that could affect litigation risk and auditor conservatism cross-sectionally. The 11 factors and their proxies are shown in Table 1. Thus, our multivariate tests examine whether an Andersen audit was a unique source of litigation risk that led successor auditors to be more conservative with their ex-Andersen clients.

III. METHOD AND RESULTS

Sample

We screen the Compustat database for U.S. firms that were audited by Andersen in 2001 and audited by one of the Big 4 in 2002. We also screen the Compustat database for U.S. firms that were audited by one of the Big 4 in 2001 and were audited by the same Big 4 auditor in 2002. We do not include clients of non-Big 4 firms to control for audit quality across successor auditors, i.e., we want any differences in accruals to reflect auditor conservatism rather than quality differences between Big 4 firms and non-Big 4 firms.[14] We also require complete data and December 31 year-ends for all our sample firms. Also, consistent with prior research (e.g., Frankel et al. 2002), we delete clients with SIC codes 6000–6999. After these screens, we have a sample that consists of 368 Andersen firms and 1,271 ongoing Big 4 clients.

Table 2 provides a breakdown of the ex-Andersen clients by industry and new auditor, and provides comparisons to the control sample of ongoing Big 4 clients. Panel A shows that ex-Andersen clients operate in 49 two-digit SIC industries and that the industry composition of the ex-Andersen sample differs from the ongoing Big 4 sample in several industries. For example, Andersen was overrepresented in the oil and gas extraction, and electric, gas and sanitary services industries while it was underrepresented in chemicals, and electrical and electronic equipment. However, there is no reason to expect that the composition of the two samples would be identical. To the extent that auditors develop industry specializations (e.g., Hogan and Jeter 1999), the market shares within an industry can be uneven.

Table 2, Panel B provides the breakdown of the ex-Andersen sample by new auditor. Of the 368 ex-Andersen clients, the largest percentage (30.7 percent, 113 clients) went to Ernst & Young, while the lowest percentage (17.1 percent, 63 clients) went to PricewaterhouseCoopers (PWC). However, on a relative basis, Deloitte & Touche (DT)

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[14] Our results are unchanged if we include ex-Andersen clients that switched to a non-Big 4 auditor.

TABLE 1
Factors Affecting Litigation Risk

Factor	Proxy	Variable Name	Data Source
Expected net benefits of legal action			
Client firm size	Log of total assets at beginning of 2002	*SIZE*	Compustat
Abnormal market returns	Cumulative abnormal return from the (0, +2) window around January 10, 2002 and the (0, +2) window around February 4, 2002	*CAR*	EVENTUS
Financial condition	Altman's Z-score equal to 3.3 * (earnings before interest and tax/total assets) + 0.999 * (net sales/total assets) + 0.6 * (market value of equity/total liabilities) + 1.2 * (working capital/total assets) + 1.4 * (retained earnings/total assets) (from Altman 1968)	*ALT-Z*	Compustat and CRSP
Incidence of shareholder litigation	Indicator variable equal to 1 if SIC is 2833–2836, 3570–3577, 7370–7374, 3600–3674, 5200–5961, 8731–8734 (based on Francis et al. 1994)	*LITIND*	Compustat
Client's incentives to manage earnings			
Leverage	Total liabilities/total assets at beginning of 2002	*LEV*	Compustat
Growth	Market value of common equity/book value of common equity at beginning of 2002	*MKTBK*	Compustat and CRSP
New equity issues	Indicator equal to 1 if new shares issued in 2002 are equal to or greater than 10 percent of the shares outstanding at the beginning of 2002 (based on Kim et al. 2003)	*NEWEQ*	CRSP
Implicit claims	Factor score based on factor analysis of membership in durable goods industry (indicator variable for SICs 150–179, 245, 250–259, 283, 301, 324–399), R&D/total assets, and labor intensity [1 – (PPE/gross assets)] (from Bowen et al. 1995; Matsumoto 2002)	*ICLAIM*	Compustat
Concerns about audit quality			
Sales growth	(Net sales 2001 – net sales 2000)/net sales 2000	*SGROW*	Compustat
Member of oil and gas or electric utility industries	Indicator equal to 1 if oil and gas or electric utility firm	*OGELEC*	Compustat
Ratio of nonaudit to audit fees	Nonaudit fees 2001/audit fees 2001	*NONAUD*	Compustat Audit Fees database

TABLE 2
Breakdowns of Ex-Andersen Clients Based on Industries and Successor Auditors

Panel A: Breakdown of Ex-Andersen Clients by Industry

Two-Digit SIC Codes	Industry Description	Ex-Andersen Clients n	Ex-Andersen Clients %	Big 4 Clients %
13	Oil and gas extraction	35	9.51	3.38
28	Chemicals and allied products	34	9.24	12.98
35	Industrial machinery and equipment	22	5.98	6.92
36	Electrical and electronic equipment	17	4.62	8.97
37	Transportation equipment	8	2.17	1.73
38	Instruments and related products	23	6.25	7.71
42	Motor freight transportation and warehousing	11	2.99	0.87
48	Communications	18	4.89	3.78
49	Electric, gas, and sanitary services	33	8.97	4.41
51	Nondurable goods	7	1.90	0.79
73	Business services	55	14.95	14.87
79	Amusement and recreational services	8	2.17	0.87
80	Health services	8	2.17	2.20
87	Engineering and management services	10	2.72	3.07
Others		79	21.47	27.46
Total	49 industries	368	100.00%	
	53 industries (1,271 firms)			100.00%

Panel B: Breakdown of Ex-Andersen Clients by Successor Auditor (i.e., Big 4 Auditor in 2002)

Successor Auditor	Ex-Andersen Clients n	Ex-Andersen Clients %	Ongoing Big 4 Clients %
Deloitte & Touche	94	25.54	17.23
Ernst & Young	113	30.71	30.37
KPMG Peat Marwick	98	26.63	19.91
PricewaterhouseCoopers	63	17.12	32.49
Total	368	100.00%	100.00%

Clients with SIC codes between 6000–6999 are deleted from the analysis.

gained the most clients—it acquired 25.5 percent of the ex-Andersen clients, which far exceeded its 17.2 percent of existing ongoing Big 4 clients. KPMG also gained clients on a relative basis. On this measure, PWC did poorly. While PWC had 32.5 percent of the existing market, it gained only 17.1 percent of the ex-Andersen clients.

Abnormal Accruals

We examine H1 by considering the levels of and changes in abnormal accruals of the ex-Andersen clients in 2002 relative to ongoing Big 4 clients. We use the Jones (1991) model to estimate abnormal accruals. However, because the Jones or modified Jones models are misspecified for firms with extreme performance (e.g., Dechow et al. 1995), following the recent literature (e.g., Menon and Williams 2004; Francis et al. 2005; Kothari et al. 2005), we also use a performance-adjusted measure of abnormal accruals.

Based on Hribar and Collins (2002), we use the direct approach to compute total accruals (*TAC*), i.e., total accruals equal income before extraordinary items (Compustat item #123) less operating cash flows adjusted for discontinued operations and extraordinary items (#308 − #124). We estimate the Jones (1991) model on a cross-sectional basis for each Fama and French (1997) industry with 20 or more firms in year t:

$$TAC/A = b_1(1/A) + b_2(\Delta SALES/A) + b_3(PPE/A) + \varepsilon \tag{1}$$

where A is the total assets for firm i at the end of year $t-1$ (#6), $\Delta SALES$ is change in sales revenues for firm i in year t (#12), and PPE is the gross property, plant, and equipment for firm i and year t (#7).

We use the residuals from Equation (1) as estimates of unadjusted abnormal accruals, which we label as *AAC*.[15] We also compute a performance-adjusted measure of abnormal accrual based on Francis et al. (2005, Appendix). Using all firms available on Compustat that have sufficient data, for each Fama and French (1997) industry, we divide the sample into deciles based on return on assets (ROA). We compute the performance-adjusted abnormal accrual (*AACADJ*) by taking the residual for firm i and subtracting the median unadjusted abnormal accrual for firm i's industry-ROA decile where the median is computed excluding firm i.[16] We compute *AAC* and *AACADJ* for 2001 and 2002 for every firm in our ex-Andersen and Big 4 samples.[17] The Pearson correlation between *AAC* and *AACADJ* is 0.878.[18,19]

Table 3 contains descriptive statistics and tests of differences for our abnormal accrual measures. Panel A provides the mean, standard deviation, and median for *AAC*, *AACADJ*, *TAC*, cash flows from operations, and ROA for the ex-Andersen clients in 2001 and 2002. The mean (median) *AAC* for the ex-Andersen clients in 2001 is −0.037 (−0.015) and the

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[15] Consistent with prior research (e.g., Francis et al. 2005; Kothari et al. 2005), throughout our analyses, we winsorize all distributions to the 1 and 99 percentiles.

[16] As Kothari et al. (2005) note, an alternative approach is to include ROA or some other performance measure in the Jones (1991) model. However, this imposes linearity on the relation between accruals and the performance measure. Our approach does not impose linearity.

[17] We use the signed residual since we are interested in the direction of the manager's earnings management. Other research uses the unsigned residual, but these studies are interested in earnings quality where the unsigned residual is an inverse measure of earnings quality (e.g., see Francis et al. 2005).

[18] We also compute two other measures of abnormal accruals to assess the reasonableness of our performance-adjusted measure. First, we compute a performance-matched abnormal accrual based on Kothari et al. (2005). For each ex-Andersen client, we identify a firm from our Big 4 sample that is in the same industry and has the closest ROA. We then take the residual from Equation (1) for our ex-Andersen client and subtract the residual from Equation (1) for its matched firm. Second, for each Andersen client, we compute a time-series performance-adjusted abnormal accrual measure by estimating the Jones (1991) model on a firm-by-firm basis for all available years up to and including 2001 as long as there are at least eight observations. We then use the estimated parameters from the firm-specific regressions to compute the predicted normal accrual for 2002. The time-series abnormal accrual in 2002 is equal to the total accrual in 2002 less the predicted normal accrual for 2002. Next, we performance-adjust the estimated time-series abnormal accrual by subtracting the median time-series abnormal accrual for the firm's industry-ROA decile. The correlation between the performance-adjusted and the performance-matched abnormal accruals is 0.502 and the correlation between the performance-adjusted and the time-series performance-adjusted abnormal accruals is 0.501, and both correlations are significant at the 0.01 level.

[19] We also ran all our tests with an unadjusted and adjusted measure of current abnormal accruals. The results are qualitatively similar.

TABLE 3
Descriptive Statistics and Tests of Differences for Abnormal Accrual Measures

Panel A: Descriptive Statistics for Ex-Andersen Clients (n = 368)

	Mean	Std. Dev.	1st Percentile	Median	99th Percentile
Ex-Andersen 2001					
Abnormal Accruals (*AAC*)	−0.037	0.140	−0.499	−0.015	0.285
Adjusted Abnormal Accruals (*AACADJ*)	−0.009	0.132	−0.432	−0.008	0.438
Total Accruals (*TAC*)	−0.108	0.129	−0.597	−0.084	0.199
Operating Cash Flows	0.072	0.193	−0.662	0.086	0.455
Return on Assets	−0.036	0.213	−0.871	0.021	0.278
Ex-Andersen 2002					
Abnormal Accruals (*AAC*)	−0.048	0.106	−0.500	−0.032	0.154
Adjusted Abnormal Accruals (*AACADJ*)	−0.016	0.104	−0.405	−0.007	0.244
Total Accruals (*TAC*)	−0.099	0.097	−0.521	−0.076	0.106
Operating Cash Flows	0.074	0.145	−0.479	0.083	0.341
Return on Assets	−0.025	0.171	−0.639	0.024	0.194
Changes in Abnormal Accrual Measures (Ex-Andersen 2002 less Ex-Andersen 2001)					
Abnormal Accruals (Δ*AAC*)	−0.009	0.144	−0.479	−0.009	0.527
Adjusted Abnormal Accruals (Δ*AACADJ*)	−0.005	0.157	−0.495	0.002	0.543

Panel B: Descriptive Statistics for Big 4 Clients (n = 1,271)

	Mean	Std. Dev.	1st Percentile	Median	99th Percentile
Big 4 2001					
Abnormal Accruals (*AAC*)	−0.043	0.144	−0.603	−0.017	0.218
Adjusted Abnormal Accruals (*AACADJ*)	−0.016	0.128	−0.474	−0.004	0.276
Total Accruals (*TAC*)	−0.110	0.135	−0.634	−0.082	0.160
Operating Cash Flows	0.039	0.186	−0.632	0.071	0.441
Return on Assets	−0.070	0.229	−0.879	0.007	0.247
Big 4 2002					
Abnormal Accruals (*AAC*)	−0.038	0.119	−0.459	−0.018	0.234
Adjusted Abnormal Accruals (*AACADJ*)	−0.007	0.113	−0.387	−0.002	0.341
Total Accruals (*TAC*)	−0.091	0.112	−0.509	−0.073	0.168
Operating Cash Flows	0.036	0.187	−0.670	0.075	0.431
Return on Assets	−0.054	0.211	−0.844	0.014	0.340

(*continued on next page*)

TABLE 3 (Continued)

	Mean	Std. Dev.	1st Percentile	Median	99th Percentile
Changes in Abnormal Accrual Measures (Big 4 2002 less Big 4 2001)					
Abnormal Accruals (ΔAAC)	0.004	0.147	−0.404	−0.003	0.566
Adjusted Abnormal Accruals ($\Delta AACADJ$)	0.008	0.156	−0.423	0.001	0.592

Panel C: Tests of Differences in Abnormal Accrual Measures

Ex-Andersen 2002 less Big 4 2002

	Abnormal Accruals (AAC)	Adjusted Abnormal Accruals ($AACADJ$)
Difference in Means	−0.009	−0.009
t-statistic	−1.471	−1.368
p-value (one-tailed)	0.071	0.086
Mann-Whitney Z	−2.320	−1.864
p-value (one-tailed)	0.010	0.031

Changes for Ex-Andersen less Changes for Big 4

	Abnormal Accruals (ΔAAC)	Adjusted Abnormal Accruals ($\Delta AACADJ$)
Difference in Means	−0.013	−0.013
t-statistic	−1.501	−1.455
p-value (one-tailed)	0.067	0.073
Mann-Whitney Z	−1.676	−0.900
p-value (one-tailed)	0.047	0.184

(continued on next page)

TABLE 3 (Continued)

Variable definitions:

AAC = unadjusted accruals computed as the residual from Equation (1):

$$TAC/A = b_1(1/A) + b_2(\Delta SALES/A) + b_3(PPE/A) + \varepsilon \qquad (1)$$

where total accruals (*TAC*) are income before extraordinary items (Compustat item #123) less operating cash flows adjusted for discontinued operations and extraordinary items (#308 − #124) (Hribar and Collins 2002), *A* is the total assets for firm *i* at the end of year $t-1$ (#6), $\Delta SALES$ is change in sales revenues for firm *i* in year *t* (#12), and *PPE* is the gross property, plant, and equipment for firm *i* and year *t* (#7). The equation is estimated on a cross-sectional basis for each Fama and French (1997) industry with 20 or more firms in year *t*;

AACADJ = performance-adjusted abnormal accruals where for each combination of Fama-French industry and year, the sample is divided into deciles based on ROA. For firm *i*, the unadjusted abnormal accruals (residual from Equation (1) above) is adjusted by subtracting the median unadjusted abnormal accrual for firm *i*'s industry-ROA decile where the median is computed excluding firm *i*;

TAC = total accruals computed as income before extraordinary items (Compustat item #123) less operating cash flows (#308) plus extraordinary items and discontinued operations (#124), scaled by total assets at the beginning of the year;

Operating Cash Flows = operating cash flows (#308) less extraordinary items and discontinued operations (#124) divided by total assets at the beginning of the year; and

Return on Assets = net income before extraordinary items divided by total assets at the beginning of the year.

All variables are winsorized to the 1 and 99 percentiles. Significance of means and medians are evaluated using a t-statistic and a Z-statistic from the Mann-Whitney test, respectively.

mean (median) for 2002 is −0.048 (−0.032). After performance adjusting, the means and medians for *AACADJ* are closer to zero. The mean (median) *AACADJ* for the ex-Andersen clients in 2001 is −0.009 (−0.008) and the mean (median) for 2002 is −0.016 (−0.007). Panel A also shows that mean and median changes in abnormal accruals between 2001 and 2002 for the ex-Andersen clients are close to zero. Three of the four changes (i.e., the two means and the median change for *AAC*) are negative.

Table 3, Panel B provides the mean, standard deviation, and median for *AAC*, *AACADJ*, *TAC*, cash flows from operations, and ROA for the ongoing Big 4 clients in 2001 and 2002. The mean (median) *AAC* for the Big 4 clients in 2001 is −0.043 (−0.017) and the mean (median) for 2002 is −0.038 (−0.018). The mean (median) for *AACADJ* for the Big 4 clients in 2001 is −0.016 (−0.004) and the mean (median) for 2002 is −0.007 (−0.002). Panel B also shows that three of the four changes in abnormal accruals for the Big 4 clients (i.e., the two means and the median change for *AACADJ*) are positive.

Table 3, Panel C provides the univariate tests of H1 for the overall sample. We first compare the level of abnormal accruals of the ex-Andersen clients in 2002 with the level of abnormal accruals of the ongoing Big 4 clients in 2002. We find that the difference is significant for *AAC* and *AACADJ* at the 0.10 level based on t-tests and at the 0.05 level based on Mann-Whitney tests. This indicates that consistent with H1, the level of unadjusted and adjusted abnormal accruals for ex-Andersen clients in 2002 is lower than the level of unadjusted and adjusted abnormal accruals for Big 4 control firms in 2002. For the changes tests that compare the changes in abnormal accruals from 2001 to 2002 for the ex-Andersen and Big 4 clients, we find that the ex-Andersen clients had bigger decreases in *AAC*, and the difference is significant at the 0.10 level based on a t-test and 0.05 level based on a Mann-Whitney test. We also find that the change in *AACADJ* is more negative for the ex-Andersen clients. This difference is significant based on a t-test, but not based on a Mann-Whitney test. Thus, at a univariate level, we find support for H1 as seven of the eight test statistics in Table 3, Panel C are negative and significant.

Multivariate Analyses

To examine differences in the level of abnormal accruals between the ex-Andersen and ongoing Big 4 clients while controlling for the 11 litigation risk factors described in Section II, we estimate the following regression model:

$$\begin{aligned} AAC, AACADJ = {} & b_0 + b_1 ANDERSEN + b_2 HOUSTON + b_3 SIZE + b_4 CAR^{*}{-1} \\ & + b_5 ALT\text{-}Z^{*}{-1} + b_6 LITIND + b_7 LEV + b_8 MKTBK \\ & + b_9 NEWEQ + b_{10} JCLAIM + b_{11} SGROW + b_{12} OGELEC \\ & + b_{13} NONAUD \end{aligned} \tag{2}$$

where *ANDERSEN* is equal to 1 if the firm is an ex-Andersen client, and *HOUSTON* is equal to 1 if the ex-Andersen client was audited by Andersen's Houston office (all other independent variables are as defined in Table 1). *ANDERSEN* examines whether abnormal accruals are significantly lower for ex-Andersen clients, and *HOUSTON* examines whether auditors were incrementally more conservative if the ex-Andersen client is a former client of Andersen's Houston office.[20] We include *HOUSTON* because there is empirical (e.g., Chaney and Philipich 2002; Krishnan 2005) and anecdotal evidence that suggests that problems at Andersen's Houston office could have extended beyond Enron.

We estimate Equation (2) using our unadjusted (*AAC*) and adjusted (*AACADJ*) measures of abnormal accruals.[21] Also, in Equation (2), we multiply *CAR* and *ALT-Z* by −1 for convenience so that for all 11 litigation risk factors, higher values represent more litigation risk. Consequently, we expect that their signs will be negative and significant if they are related to auditor conservatism.

In addition to our levels specification, we also run a changes model that examines whether the change in abnormal accruals from 2001 to 2002 is different for ex-Andersen clients compared to the ongoing Big 4 clients. Specifically, we first difference the continuous variables in Equation (2) yielding the following model:

$$\begin{aligned}\Delta AAC, \Delta AACADJ = {} & b_0 + b_1 ANDERSEN + b_2 HOUSTON + b_3 \Delta SIZE \\ & + b_4 CAR^{*}{-1} + b_5 \Delta ALT\text{-}Z^{*}{-1} + b_6 LITIND + b_7 \Delta LEV \\ & + b_8 \Delta MKTBK + b_9 NEWEQ + b_{10} \Delta ICLAIM \\ & + b_{11} \Delta SGROW + b_{12} OGELEC + b_{13} \Delta NONAUD \qquad (3)\end{aligned}$$

where *ANDERSEN* and *HOUSTON* are variables of interest and the expected sign of all the variables is negative.

Table 4 provides descriptive statistics for the litigation risk variables that we use in our multivariate tests. Because they represent the *ex ante* litigation risk (i.e., the pre-audit assessment of litigation risk) before the 2002 audit was conducted, we use the lagged values (i.e., 2001 values) for these variables except for *CAR* and *NEWEQ*. *CAR* relates to event dates in 2002. *NEWEQ* reflects new equity issues during 2002.

We find differences between the ex-Andersen and ongoing Big 4 clients on a number of dimensions.[22] For example, the ex-Andersen clients have lower cumulative abnormal returns around January 10, 2002 and February 4, 2002 and lower Altman Z-scores than the ongoing Big 4 clients, which indicates higher litigation risk.[23] Additionally, we find evidence that the ex-Andersen clients are larger, have more debt, have higher nonaudit fee ratios, and are more likely to be in the oil and gas or electric utility industries, which is consistent with higher litigation risk. On the other hand, ex-Andersen clients have lower implicit claims, which suggests lower litigation risk. Thus, it appears that, on balance, Andersen's client base has a different litigation risk profile than the client base of the Big 4. Our multivariate tests examine whether being audited by Andersen is viewed as a separate

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[20] To determine if an Andersen client was audited by the Houston office, using Compustat, we first identified each ex-Arthur Andersen clients' corporate headquarters or home office. We then narrowed our search by focusing on 87 ex-Andersen clients that have a headquarters or home office in Texas or in one of the states that neighbor Texas, i.e., New Mexico, Oklahoma, Arkansas, Mississippi, and Louisiana. We examined these clients' 10-K reports and determined whether they were audited by Arthur Andersen's Houston office.

[21] To the extent that *ANDERSEN* is uncorrelated with performance, the model using unadjusted abnormal accruals should provide a more powerful test of H1 since performance adjusting could introduce additional noise into the abnormal accrual measure.

[22] We use a one-tailed test for *CAR* because based on Chaney and Philipich (2002), we expect a negative difference. We do not have any priors about the characteristics of the ex-Andersen clients versus the Big 4 clients on the other dimensions, and we use two-tailed tests for the remaining variables.

[23] Our results for *CAR* are consistent with Chaney and Philipich (2002) who find more negative cumulative abnormal returns for the ex-Andersen clients around the January 10 and February 4 event dates compared to clients of the Big 4.

TABLE 4
Descriptive Statistics for Litigation Risk Factors for the Ex-Andersen and Big 4 Samples

	Ex-Andersen			Big 4			Difference (Ex-Andersen – Big 4)	
	n	Mean	Median	n	Mean	Median	t-statistic	M-W Z
Panel A: Benefits Variables								
SIZE	368	2.705	2.620	1,271	2.569	2.496	2.765***	2.863***
CAR	368	−0.035	−0.025	1,271	−0.027	−0.020	−1.487*	−1.765**
ALT-Z	368	4.217	2.566	1,271	5.831	3.014	−1.664*	−2.297**
							χ^2–statistic	
LITIND	368	0.277	0.000	1,271	0.390	0.000	15.744***	
Panel B: Incentive Variables								
LEV	368	0.528	0.536	1,271	0.470	0.465	3.644***	3.967***
MKTBK	368	2.705	1.767	1,271	2.759	1.999	−0.246	−0.810
ICLAIM	368	−0.158	−0.133	1,271	0.120	0.136	−4.480***	−4.401***
							χ^2–statistic	
NEWEQ	368	0.141	0.000	1,271	0.144	0.000	0.017	
Panel C: Concern Variables								
SGROW	368	0.179	0.022	1,271	0.172	0.029	0.135	0.419
NONAUD	368	1.652	1.176	1,271	1.408	0.928	2.492**	3.020***
							χ^2–statistic	
OGELEC	368	0.122	0.000	1,271	0.048	0.000	26.036***	

(continued on next page)

TABLE 4 (Continued)

***, **, * Denote significance at the 0.01, 0.05, and 0.10 levels, respectively. M-W Z is the Z-statistic for the Mann-Whitney test. t- and Z-tests are two-tailed except for *CAR*.

Variable definitions:

SIZE = log of total assets (in millions) at beginning of 2002;
CAR = cumulative abnormal return from the (0, +2) window around January 10, 2002 and (0, +2) window around February 4, 2002;
ALT-Z = Altman's Z-score at beginning of 2002;
LITIND = 1 if high litigation industry based on Francis et al. (1994), and 0 otherwise;
LEV = total liabilities/total assets at beginning of 2002;
MKTBK = market value of equity/book value of equity at beginning of 2002;
ICLAIM = implicit claims (based on Matsumoto 2002) at beginning of 2002;
NEWEQ = 1 if new shares issued in 2002 are equal to or greater than 10 percent of existing shares, and 0 otherwise;
SGROW = $(sales_{2001} - sales_{2000})/sales_{2000}$;
OGELEC = 1 if oil and gas or electric utility firm, and 0 otherwise; and
NONAUD = nonaudit fees/audit fees for 2001.

source of litigation risk that affects auditor conservatism after controlling for these other differences in litigation risk.

Table 5 provides the pairwise Pearson and Spearman correlation coefficients between our abnormal accrual measures and the independent variables. We discuss the Pearson correlations unless there are important differences between the two. The unadjusted accruals (*AAC*) and adjusted accruals (*AACADJ*) are negatively and significantly correlated with *ANDERSEN*. These relations are significant based on the Spearman correlation, but are not significant based on the Pearson correlation.

Further, *AAC* and *AACADJ* are each significantly correlated with at least six of 11 litigation risk factors. However, the degree of correlation can differ noticeably between the *AAC* and *AACADJ* measures. For example, the correlation between *AAC* and *SIZE* is 0.113, which is significant at the 0.01 level, but the correlation between *AACADJ* and *SIZE* is −0.005, which is not significant. This suggests that, at least, some of the litigation factors are correlated with performance.[24]

We also find that *ANDERSEN* is significantly correlated with seven of the 11 litigation risk factors, which indicates that it is important to control for the other litigation risk factors. However, the correlations are not excessively high (the highest is between *ANDERSEN* and *OGELEC* at 0.126). This suggests that *ANDERSEN* captures a unique element of litigation risk that is not associated with the other litigation risk factors. In addition, 40 of the 55 pairwise correlations between the litigation risk factors are significant suggesting that the litigation risk is both complex and multidimensional.

Table 6 provides the results for Equation (2) for both the unadjusted (*AAC*) and adjusted (*AACADJ*) accruals. In both models, the variable of interest, *ANDERSEN*, is negative and significant at the 0.05 level. This suggests that after controlling for the level of litigation risk, auditors required more income-reducing unadjusted and performance-adjusted abnormal accruals for their ex-Andersen clients. This supports H1. However, *HOUSTON* is not significant, which indicates that auditors were not incrementally more conservative with former clients of Andersen's Houston office.

Examining the results for the *AAC* columns, we find that six of the 11 litigation risk factors have negative and significant coefficients as expected. Also, the model has an adjusted R^2 of 8.3 percent. For the model using *AACADJ* as the dependent variable, five of the 11 litigation risk variables are negative and significant, and the adjusted R^2 is 4.5 percent. Thus, the model using *AAC* has more explanatory power, but the extra power comes at a cost. Specifically, since a number of the litigation risk factors are correlated with current performance, interpreting the coefficients of the litigation risk factors in the *AAC* model is difficult as they may be capturing either litigation risk or performance effects. Thus, we focus on the *AACADJ* model to interpret the coefficients of the litigation risk variables.

In the *AACADJ* model, CAR^*_{-1} has a negative coefficient and is significant at the 0.01 level, based on a one-tailed test. This indicates that where there were more concerns about the credibility of the financial statement and, hence, a higher probability of litigation, successor auditors were more conservative with their ex-Andersen and ongoing clients. Also, *SIZE* is negative and significant at the 0.01 level. Since large firms are more likely litigation targets (e.g., Lys and Watts 1994), this is consistent with the view that a higher probability of litigation induces more auditor conservatism. The coefficient for *ICLAIM* is negative and

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[24] We find that eight of the 11 variables are significantly correlated with current ROA. This suggests that when *AAC* is the dependent variable, the coefficients for the litigation risk factors need to be interpreted with care because they may be capturing performance effects. However, this should not bias our findings for *ANDERSEN*.

TABLE 5
Pearson and Spearman Correlation Coefficients

	AAC	*AACADJ*	*ANDERSEN*	*HOUSTON*	*SIZE*	*CAR*−1*	*ALT-Z*−1*	*LITIND*
AAC		**0.792**	**−0.057**	0.017	**0.083**	**−0.126**	0.026	**−0.182**
AACADJ	**0.878**		−0.046	0.004	−0.040	**−0.072**	**0.107**	−0.035
ANDERSEN	−0.034	−0.032		**0.241**	**0.071**	0.044	**0.057**	**−0.098**
HOUSTON	0.016	−0.002	**0.241**		**0.052**	**0.055**	0.012	−0.048
SIZE	**0.113**	−0.005	**0.068**	0.040		**−0.093**	**0.149**	**−0.313**
CAR−1*	**−0.142**	**−0.096**	0.037	**0.050**	**−0.053**		0.040	**0.065**
ALT-Z−1*	0.036	**0.121**	**0.051**	0.006	**0.131**	−0.030		**−0.205**
LITIND	**−0.203**	**−0.071**	**−0.098**	**−0.048**	**−0.300**	**0.053**	**−0.192**	
LEV	**0.173**	**0.150**	**0.090**	0.035	**0.403**	−0.005	**0.523**	**−0.316**
MKTBK	−0.014	−0.009	−0.006	−0.039	**−0.047**	−0.006	**−0.284**	**0.130**
NEWEQ	−0.042	−0.001	−0.003	0.029	**−0.069**	0.037	0.048	**0.055**
ICLAIM	**−0.064**	**−0.110**	**−0.110**	**−0.072**	**−0.272**	0.003	**−0.302**	**0.351**
SGROW	**−0.083**	**−0.078**	0.003	0.000	−0.045	**0.076**	**−0.106**	**0.071**
OGELEC	0.011	0.002	**0.126**	**0.102**	**0.167**	0.031	**0.106**	**−0.199**
NONAUD	−0.022	**−0.054**	**0.061**	−0.007	**0.387**	−0.007	0.009	**−0.057**

	LEV	*MKTBK*	*NEWEQ*	*ICLAIM*	*SGROW*	*OGELEC*	*NONAUD*
AAC	**0.203**	−0.015	−0.009	−0.030	−0.022	−0.015	−0.038
AACADJ	0.123	−0.026	−0.001	**−0.087**	−0.044	−0.012	**−0.075**
ANDERSEN	**0.098**	−0.020	−0.003	**−0.109**	0.010	**0.126**	**0.075**
HOUSTON	0.036	−0.043	0.029	**−0.067**	**0.043**	**0.102**	−0.013
SIZE	**0.477**	0.012	0.002	**−0.273**	**0.056**	**0.165**	**0.453**
CAR−1*	−0.044	0.020	0.041	0.007	**0.088**	**0.055**	−0.043
ALT-Z−1*	**0.613**	**−0.457**	**0.121**	**−0.341**	**−0.096**	**0.189**	−0.032
LITIND	**−0.375**	**0.208**	**0.055**	**0.348**	0.004	**−0.199**	−0.048
LEV		**−0.174**	**0.062**	**−0.413**	−0.016	**0.165**	**0.136**

(*continued on next page*)

TABLE 5 (Continued)

MKTBK	**−0.148**		0.026	**0.194**	**0.220**	**−0.084**	**0.125**
NEWEQ	**0.070**	0.020		0.031	**0.083**	0.034	−0.039
ICLAIM	**−0.381**	**0.140**	0.028		**−0.082**	**−0.375**	−0.010
SGROW	**−0.056**	**0.083**	**0.102**	**0.072**		**0.118**	**0.069**
OGELEC	**0.143**	**−0.067**	0.034	**−0.391**	0.011		0.009
NONAUD	**0.117**	0.015	−0.016	−0.042	0.008	0.046	

Pearson (Spearman) correlations are shown below (above) the diagonal. Coefficients in bold only are significant at the .05 level (two-tailed).

Variable definitions:

Abnormal Accruals (AAC) = unadjusted accruals computed as the residual from Equation (1):

$$TAC/A = b_1(1/A) + b_2(\Delta SALES/A) + b_3(PPE/A) + \varepsilon \quad (1)$$

where total accruals (*TAC*) are income before extraordinary items (Compustat item #123) less operating cash flows adjusted for discontinued operations and extraordinary items (#308 − #124) (Hribar and Collins 2002), *A* is the total assets for firm *i* at the end of year $t-1$ (#6), *ΔSALES* is change in sales revenues for firm *i* in year *t* (#12), and *PPE* is the gross property, plant, and equipment for firm *i* and year *t* (#7). The equation is estimated on a cross-sectional basis for each Fama and French (1997) industry with 20 or more firms in year *t*;

Adjusted Abnormal Accruals (AACADJ) = performance-adjusted abnormal accruals where for each combination of Fama-French industry and year, the sample is divided into deciles based on ROA. For firm *i*, the unadjusted abnormal accruals (residual from Equation (1) above) is adjusted by subtracting the median unadjusted abnormal accrual for firm *i*'s industry-ROA decile where the median is computed excluding firm *i*;

ANDERSEN = 1 for ex-Andersen clients in 2002, and 0 otherwise;
HOUSTON = 1 for clients of Andersen's Houston office in 2001, and 0 otherwise;
SIZE = log of total assets (in millions) at beginning of 2002;
CAR = cumulative abnormal return from the (0, +2) window around January 10, 2002 and (0, +2) window around February 4, 2002;
ALT-Z = Altman's Z-score at beginning of 2002;
LITIND = 1 if high litigation industry based on Francis et al. (1994), and 0 otherwise;
LEV = total liabilities/total assets at beginning of 2002;
MKTBK = market value of equity/book value of equity at beginning of 2002;
ICLAIM = implicit claims (based on Matsumoto 2002) at beginning of 2002;
NEWEQ = 1 if new shares issued in 2002 are equal to or greater than 10 percent of existing shares, and 0 otherwise;
SGROW = $(sales_{2001} - sales_{2000})/sales_{2000}$;
OGELEC = 1 if oil and gas or electric utility firm, and 0 otherwise; and
NONAUD = nonaudit fees/audit fees for 2001.

TABLE 6
Parameter Estimates for Regressions of 2002 Unadjusted and Adjusted Abnormal Accruals on Litigation Risk Variables for Ex-Andersen and Big 4 Clients
Levels Specification

$$\begin{aligned} AAC, AACADJ = {} & b_0 + b_1 ANDERSEN + b_2 HOUSTON + b_3 SIZE + b_4 CAR^{*}{-1} + b_5 ALT\text{-}Z^{*}{-1} \\ & + b_6 LITIND + b_7 LEV + b_8 MKTBK + b_9 NEWEQ + b_{10} ICLAIM \\ & + b_{11} SGROW + b_{12} OGELEC + b_{13} NONAUD \end{aligned}$$

		Model 1 *AAC*		Model 2 *AACADJ*	
Variable	**Exp. Sign**	**Estimate**	**t-statistic**	**Estimate**	**t-statistic**
Intercept	+/−	−0.064	−5.39***	0.004	0.34
Test Variables					
ANDERSEN	−	−0.016	−2.27**	−0.012	−1.75**
HOUSTON	−	0.023	1.05	0.007	0.34
Control Variables					
SIZE	−	0.004	0.99	−0.010	−2.53***
*CAR**−1	−	−0.162	−5.28***	−0.111	−3.71***
*ALT-Z**−1	−	−0.001	−2.82***	0.001	1.30
LITIND	−	−0.040	−6.29***	−0.005	−0.89
LEV	−	0.077	5.72	0.057	4.34
MKTBK	−	0.000	0.51	0.001	1.40
NEWEQ	−	−0.011	−1.32*	−0.002	−0.25
ICLAIM	−	0.004	1.27	−0.008	−2.59***
SGROW	−	−0.008	−2.43***	−0.008	−2.36***
OGELEC	−	−0.007	−0.60	−0.014	−1.15
NONAUD	−	−0.004	−2.26**	−0.002	−1.63*
Adj. R^2		0.083		0.045	
F-statistic		12.36***		6.92***	
n		1639		1639	

***, **, * Denote significance at the .01, .05, and .10 levels, respectively. Tests are one-tailed.

Variable definitions:

AAC = unadjusted abnormal accruals;
AACADJ = performance-adjusted abnormal accruals;
ANDERSEN = 1 for ex-Andersen clients in 2002, and 0 otherwise;
HOUSTON = 1 for clients of Andersen's Houston office in 2001, and 0 otherwise;
SIZE = log of total assets (in millions) at beginning of 2002;
CAR = cumulative abnormal return from the (0, +2) window around January 10, 2002 and (0, +2) window around February 4, 2002;
ALT-Z = Altman's Z-score at beginning of 2002;
LITIND = 1 if high litigation industry based on Francis et al. (1994), and 0 otherwise;
LEV = total liabilities/total assets at beginning of 2002;
MKTBK = market value of equity/book value of equity at beginning of 2002;
ICLAIM = implicit claims (based on Matsumoto 2002) at beginning of 2002;
NEWEQ = 1 if new shares issued in 2002 are equal to or greater than 10 percent of existing shares, and 0 otherwise;
SGROW = $(sales_{2001} - sales_{2000})/sales_{2000}$;
OGELEC = 1 if oil and gas or electric utility firm, and 0 otherwise; and
NONAUD = nonaudit fees/audit fees for 2001.

is significant at the 0.01 level. When managers have incentive to manage earnings upward in response to implicit claims, auditors are more conservative.

In terms of audit quality, *SGROW* is negatively related to *AACADJ,* suggesting that when sales growth is high, auditors constrain accruals. Also, the negative and significant coefficient on *NONAUD* indicates that higher nonaudit fee ratios are inversely related to abnormal accruals and positively related to auditor conservatism. This is interesting as Frankel et al. (2002) find a positive relation between the fee ratio and abnormal accruals and other recent studies find no relation (e.g., Chung and Kallapur 2003). Our results are consistent with Larcker and Richardson (2004) and suggest that since nonaudit fees can lead to (or lead to a perception of) lower auditor independence and lower audit quality, auditors of clients with high lagged nonaudit fees are likely to be more conservative to offset the higher litigation risk.

Last, *LEV* is highly significant but is incorrectly signed. The positive coefficient suggests that highly leveraged firms have higher abnormal accruals and that auditors do not seem to constrain them. Thus, rather than measuring litigation risk, *LEV* could be detecting upward earnings management activity of high-debt firms.

Since a number of the independent variables exhibit skewness, we also estimate Equation (2) using rank regressions. The results (untabulated) are qualitatively unchanged. Also, because our Enron-motivated variables may be capturing some of the litigation risk that we want *ANDERSEN* to capture (e.g., as seen in Table 4, ex-Andersen clients have lower CARs, higher nonaudit fees, and are more likely to be in the oil and gas or electric utility industries), we also estimated Equation (2) without $CAR^*{-1}$, *SGROW*, *OGELEC*, and *NONAUD*. The results (untabulated) indicate that *ANDERSEN* is negatively signed and significant at the 0.01 level ($t = -2.611$) in the *AAC* model and is negatively signed and significant at the 0.05 level ($t = -2.062$) in the *AACADJ* model.

Table 7 contains the results for the changes model, i.e., Equation (3). Consistent with H1, *ANDERSEN* is significant and negative in both models at the 0.10 level, based on a one-tailed test, while *HOUSTON* is not significant. The adjusted R^2s for the ΔAAC and $\Delta AACADJ$ models are 6.5 percent and 5.8 percent, respectively.

Again, we focus on the adjusted abnormal accrual model to interpret the litigation risk variables. Five of the 11 variables are significant and negatively signed. $CAR^*{-1}$, $\Delta SIZE$, and $\Delta ICLAIM$ are significant and negatively signed similar to Table 6. Auditors appear to react to positive changes in firm size and positive changes in implicit claims by reducing allowable accruals. Also, $\Delta ALT\text{-}Z^*{-1}$ and ΔLEV are both significant at the 0.01 level with negative signs. Thus, as financial condition weakens and as leverage increases, auditors respond by becoming more conservative. The results for *LEV* (a positive coefficient) and ΔLEV (a negative coefficient) may seem contradictory but suggest that while high leverage firms have higher levels of abnormal accruals (perhaps reflecting earnings management), auditors constrain abnormal accruals when the change in leverage is large and positive.[25]

Combined, we find evidence that successor auditors treated their ex-Andersen clients differently from their ongoing clients, even after controlling for other litigation factors, which suggests that the successor auditors saw an Andersen audit as a unique source of litigation risk. However, auditors were not more conservative with their ex-Andersen clients that were audited by Andersen's Houston office.

Additional Analyses

We conduct two additional analyses. Since our results for *ANDERSEN* are similar whether we use the unadjusted or adjusted abnormal accrual measure, we only discuss the results for the adjusted abnormal accrual measure in this section.

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[25] We also re-ran our changes model (for ΔAAC and $\Delta AACADJ$) using the average of 2000 and 2001 in place of 2001. While the coefficients for *ANDERSEN* are negative, they are not significant at conventional levels.

TABLE 7
Parameter Estimates for Regressions of Unadjusted and Adjusted Abnormal Accruals on Litigation Risk Variables for Ex-Andersen and Big 4 Clients
Changes Specification

$$\begin{aligned}\Delta AAC,\ \Delta AACADJ = {} & b_0 + b_1 ANDERSEN + b_2 HOUSTON + b_3 \Delta\, SIZE + b_4 CAR^{*}{-1} \\ & + b_5 \Delta ALT\text{-}Z^{*}{-1} + b_6 LITIND + b_7 \Delta LEV + b_8 \Delta MKTBK + b_9 NEWEQ \\ & + b_{10} \Delta ICLAIM + b_{11} \Delta SGROW + b_{12} OGELEC + b_{13} \Delta NONAUD\end{aligned}$$

		Model 1 ΔAAC		Model 2 $\Delta AACADJ$	
Variable	**Exp. Sign**	**Estimate**	**t-statistic**	**Estimate**	**t-statistic**
Intercept	+/−	−0.000	−0.03	0.012	2.07**
Test Variables					
ANDERSEN	−	−0.012	−1.40*	−0.015	−1.54*
HOUSTON	−	−0.013	−0.45	−0.002	−0.09
Control Variables					
$\Delta SIZE$	−	−0.076	−2.30***	−0.064	−1.81**
$CAR^{*}-1$	−	−0.039	−0.99	−0.060	−1.42*
$\Delta ALT\text{-}Z^{*}-1$	−	−0.000	−1.08	−0.002	−2.44***
LITIND	−	0.022	2.84	0.014	1.65
ΔLEV	−	−0.264	−10.03***	−0.258	−9.15***
$\Delta MKTBK$	−	0.000	0.46	0.000	0.39
NEWEQ	−	0.000	0.03	0.002	0.19
$\Delta ICLAIM$	−	−0.057	−2.02*	−0.040	−1.32*
$\Delta SGROW$	−	−0.002	−0.51	0.005	1.04
OGELEC	−	0.034	2.28	0.019	1.20
$\Delta NONAUD$	−	0.000	0.11	−0.000	−0.09
Adj. R^2		0.065		0.058	
F-statistic		9.73***		8.77***	
n		1639		1639	

***, **, * Denote significance at the .01, .05, and .10 levels, respectively. Tests are one-tailed.

Variable definitions:

AAC = unadjusted abnormal accruals;

AACADJ = performance-adjusted abnormal accruals;

ANDERSEN = 1 for ex-Andersen clients in 2002, and 0 otherwise;

HOUSTON = 1 for clients of Andersen's Houston office in 2001, and 0 otherwise;

SIZE = log of total assets (in millions) at beginning of 2002;

CAR = cumulative abnormal return from the (0, +2) window around January 10, 2002 and (0, +2) window around February 4, 2002;

ALT-Z = Altman's Z-score at beginning of 2002;

LITIND = 1 if high litigation industry based on Francis et al. (1994), and 0 otherwise;

LEV = total liabilities/total assets at beginning of 2002;

MKTBK = market value of equity/book value of equity at beginning of 2002;

ICLAIM = implicit claims (based on Matsumoto 2002) at beginning of 2002;

NEWEQ = 1 if new shares issued in 2002 are equal to or greater than 10 percent of existing shares, and 0 otherwise;

SGROW = $(sales_{2001} - sales_{2000})/sales_{2000}$;

OGELEC = 1 if oil and gas or electric utility firm, and 0 otherwise; and

NONAUD = nonaudit fees/audit fees for 2001.

Results by Big 4 Auditor

Similar to Chaney and Philipich (2002), we further analyze the data by examining each Big 4 successor auditor independently. Specifically, we estimate Equations (2) and (3) for ex-Andersen firms that were audited by EY, DT, KPMG, and PWC. Table 8 contains abbreviated results. For Equation (2) when *AACADJ* is the dependent variable (i.e., Panel A), in all four models, *ANDERSEN* is negative, but it is only significant for the model estimated with EY clients. For Equation (3) (i.e., Panel B), *ANDERSEN* is negative and significant for EY clients and for KPMG clients. For Equation (3), we also find a significant and negative coefficient for *HOUSTON* in the model for PWC clients. This suggests that while PWC was not more conservative with their Andersen clients generally, it was incrementally more conservative with ex-Andersen clients that had been audited by Andersen's Houston office.

We conjecture that the strong results for EY might be a by-product of its client selection policies and/or its own litigation history. Because Ernst & Young acquired the most ex-Andersen clients of all Big 4 firms (30.7 percent), it is possible that Ernst & Young may have been less selective in accepting clients and, as a rational response, may have compensated by taking a more conservative stance with its new clients. Also, Ernst & Young had litigation problems of its own. In November 2002, the Federal Deposit Insurance Corp. sued Ernst & Young for $2.2 billion over the failure of Superior Bank (e.g., Ernst & Young sued ... 2002), and in early 2003, Ernst & Young was in the news for being the auditor of HealthSouth Corp. which was accused by the U.S. government for overstating earnings by $2.5 billion over several years (e.g., Ernst & Young's woes ... 2003). Moreover, in 1999, Ernst & Young paid $335 million to settle a lawsuit involving Cendant Corp., which was one of its audit clients. This was the biggest accounting settlement ever prior to 2002 (Hawkins and Cohen 2003). Thus, given its own history, Ernst & Young may have been more sensitized to litigation risk.

Negative Special Items

In addition to examining abnormal accruals, we also examine negative special items and negative changes in special items. Because any test involving the Jones model is a joint test of the hypothesized effect and the ability of the Jones model to detect managed accruals, we use special items as a more direct way to measure accounting discretion. If the successor auditor was concerned about overstatements in the financial statements previously audited by Andersen, then the auditor could require that the client write-down those items to more acceptable levels. Also, because special items are highly visible, they can be used by new auditors to send signals that they are actively trying to clean up the financial statements, and this could be useful in reducing litigation risk. DeFond and Subramanyam (1998) examine special items in their supplemental tests and find that there are more income-reducing special items when litigation risk is high.

We use negative special items scaled by lagged total assets (*NEGSI*) as the dependent variable and re-estimate Equation (2). We also use negative changes (i.e., decreases) in special items scaled by lagged total assets ($NEG\Delta SI$) and re-estimate Equation (3). We focus on negative special items and negative changes in special items because we are interested in situations where auditors require write-downs. Since *NEGSI* and $NEG\Delta SI$ are censored at zero (e.g., when there are no special items or when special items are positive, $NEGSI = 0$), we use Tobit regression (e.g., Ramanathan 1992). Table 9 contains these results. Neither *ANDERSEN* nor *HOUSTON* is significant in either model, which indicates that Big 4 successor auditors did not require larger negative special items or more negative changes in special items. While contrary to DeFond and Subramanyam (1998), this suggests

TABLE 8
Parameter Estimates for Regressions of 2002 Adjusted Abnormal Accruals on Litigation Risk Variables for Ex-Andersen and Big 4 Clients by Big 4 Successor Auditor

Panel A: Levels Specification

		DT		EY		KPMG		PWC	
Variable	**Exp. Sign**	**Estimate**	**t-statistic**	**Estimate**	**t-statistic**	**Estimate**	**t-statistic**	**Estimate**	**t-statistic**
Test Variables									
ANDERSEN	–	−0.007	−0.51	−0.024	−1.81**	−0.004	−0.28	−0.004	−0.25
HOUSTON	–	0.019	0.44	0.040	1.16	−0.047	−0.55	−0.039	−0.89
Adj. R^2		0.028		0.028		0.014		0.026	
n		313		499		351		476	

Panel B: Changes Specification

		DT		EY		KPMG		PWC	
Variable	**Exp. Sign**	**Estimate**	**t-statistic**	**Estimate**	**t-statistic**	**Estimate**	**t-statistic**	**Estimate**	**t-statistic**
Test Variables									
ANDERSEN	–	0.015	0.85	−0.027	−1.43*	−0.031	−1.53*	−0.009	−0.44
HOUSTON	–	−0.017	−0.28	0.059	1.17	−0.013	−0.11	−0.103	−1.75**
Adj. R^2		0.054		0.059		0.061		0.084	
n		313		499		351		476	

**, * Denote significance at the .05 and .10 levels, respectively. Tests are one-tailed.

Panel A reflects regression results for Equation (2) estimated separately by successor auditor. Panel B reflects regression results for Equation (3) estimated separately by successor auditor. For economy, only the coefficients for *ANDERSEN* and *HOUSTON* are reported.

Variable definitions:

ANDERSEN = 1 for ex-Andersen clients in 2002, and 0 otherwise; and

HOUSTON = 1 for clients of Andersen's Houston office in 2001, and 0 otherwise.

TABLE 9

Parameter Estimates for Tobit Regressions of 2002 Negative Special Items on Litigation Risk Variables for Ex-Andersen and Big 4 Clients

Variable	Exp. Sign	Model 1 *NEGSI* Estimate	Model 1 *NEGSI* χ^2-statistic	Variable	Exp. Sign	Model 2 *NEGΔSI* Estimate	Model 2 *NEGΔSI* χ^2-statistic
Intercept	+/−	0.031	8.11***	Intercept	+/−	0.063	127.20***
Test Variables				**Test Variables**			
ANDERSEN	−	0.002	0.06	*ANDERSEN*	−	−0.010	1.63
HOUSTON	−	0.026	1.47	*HOUSTON*	−	−0.004	0.03
Control Variables				**Control Variables**			
SIZE	−	−0.021	32.32***	Δ*SIZE*	−	0.116	16.91***
*CAR**−1	−	−0.096	12.63***	*CAR**−1	−	−0.071	4.27**
*ALT-Z**−1	−	−0.002	22.30***	Δ*ALT-Z**−1	−	−0.001	2.16
LITIND	−	−0.025	18.71***	*LITIND*	−	−0.003	0.13
LEV	−	0.074	34.20***	Δ*LEV*	−	−0.183	58.90***
MKTBK	−	0.001	0.49	Δ*MKTBK*	−	0.002	4.22**
NEWEQ	−	−0.016	5.05**	*NEWEQ*	−	−0.027	9.25***
ICLAIM	−	−0.007	6.81**	Δ*ICLAIM*	−	−0.051	4.42**
SGROW	−	0.002	0.25	Δ*SGROW*	−	−0.003	0.53
OGELEC	−	0.054	17.94***	*OGELEC*	−	0.034	5.56**
NONAUD	−	−0.001	0.13	Δ*NONAUD*	−	−0.003	1.17
Wald χ^2		140.879***		Wald χ^2		134.781***	
Pseudo R^2		0.065		Pseudo R^2		0.093	
n		1639		n		1639	

(continued on next page)

TABLE 9 (Continued)

***, **, * Denote significance at the .01, .05, and .10 levels, respectively. Tests are one-tailed.
Model 1 is based on Equation (2). Model 2 is based on Equation (3).
Variable definitions:

NEGSI = negative special items scaled by lagged total assets, and 0 for positive special items and no special items;
NEGΔSI = negative change in special items from 2001 to 2002 scaled by lagged total assets, and 0 for positive changes and no change;
ANDERSEN = 1 for ex-Andersen clients in 2002, and 0 otherwise;
HOUSTON = 1 for clients of Andersen's Houston office in 2001, and 0 otherwise;
SIZE = log of total assets (in millions) at beginning of 2002;
CAR = cumulative abnormal return from the (0, +2) window around January 10, 2002 and (0, +2) window around February 4, 2002;
ALT-Z = Altman's Z-score at beginning of 2002;
LITIND = 1 if high litigation industry based on Francis et al. (1994), and 0 otherwise;
LEV = total liabilities/total assets at beginning of 2002;
MKTBK = market value of equity/book value of equity at beginning of 2002;
ICLAIM = implicit claims (based on Matsumoto 2002) at beginning of 2002;
NEWEQ = 1 if new shares issued in 2002 are equal to or greater than 10 percent of existing shares, and 0 otherwise;
SGROW = $(sales_{2001} - sales_{2000})/sales_{2000}$;
OGELEC = 1 if oil and gas or electric utility firm, and 0 otherwise; and
NONAUD = nonaudit fees/audit fees for 2001.

that the more negative abnormal accruals found in the earlier tests are attributable to auditors imposing tighter thresholds for accruals for their ex-Andersen clients rather than having them correct for past misstatements.[26]

IV. CONCLUSION

We examine the conservatism of Big 4 successor auditors in relation to clients that they acquired from Andersen in 2002. To the extent that the Andersen-Enron affair increased concerns about the credibility of the financial statements and raised the litigation risk of other Andersen clients, we expect that the successor auditor would have incentive to require more income-reducing accounting choices for their ex-Andersen clients in 2002. We use unadjusted and adjusted measures of abnormal accruals, and we conduct tests that compare the levels and changes of the abnormal accruals between the ex-Andersen clients and a control group of ongoing Big 4 clients. In addition, we identify 11 factors related to litigation risk, and we use these to control for other factors that might be affecting auditor conservatism cross-sectionally. Overall, we find evidence of auditor conservatism in both our univariate and multivariate analyses. This suggests that successor auditors viewed an Andersen audit as a unique source of risk. However, our results are strongest for clients of Ernst & Young, which suggests that not all auditors were equally conservative. Finally, we do not find that successor auditors were incrementally more conservative with Andersen's former Houston clients.

A limitation of our study is the possible confounding effect of the enactment of the Sarbanes-Oxley Act of 2002. Several of its provisions were aimed at improving the quality of audits (see footnote 12). Thus, an alternative explanation is that the lower abnormal accruals for the ex-Andersen clients were driven by legal requirements rather than auditor conservatism. However, since Sarbanes-Oxley affected both the ex-Andersen and Big 4 clients, this argument could only apply if the legislation affected the ex-Andersen firms more severely.

In addition, prior research has only been moderately successful in identifying characteristics of litigation risk. One reason may be that litigation against auditors is relatively rare. In any case, our litigation risk variables are crude and measured with noise. Further, some of our litigation risk variables are open to alternative interpretations. For example, firms issue equity to fund growth, and this can lead to normal increases in working capital and accruals. Thus, our new equity variable might not be capturing earnings management incentives. Also, the R^2s for our regression models are modest, and as a result, the possibility of correlated omitted variables cannot be ruled out. Finally, while we use unadjusted and performance-adjusted measures of abnormal accruals, our abnormal accrual measures are only as good as the models used to estimate them.

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[26] We also examined whether successor auditors required more financial restatements for their ex-Andersen clients. Using the keyword "restate," we searched the Lexis-Nexis database for financial statement restatements during fiscal years 2001 and 2002. We found that in 2001, our Big 4 sample had 14 restatements and the ex-Andersen sample had four restatements. The rates of restatement were both 1.1 percent. However, in 2002, 35 firms in the Big 4 group (2.8 percent) restated and 27 ex-Andersen firms (7.3 percent) restated, which suggests that successor auditors were more vigorous in requiring restatements for their ex-Andersen clients. However, because restatements can be initiated by government (e.g. the SEC), the company itself, or the auditor, we cannot be sure all 27 restatements of ex-Andersen firms were auditor-initiated. Also, some of the 2002 restatement could have been initiated by Andersen before it was replaced. We suggest this as an area for future research.

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THE ACCOUNTING REVIEW
Vol. 81, No. 1
2006
pp. 83–112

The Reputational Penalty for Aggressive Accounting: Earnings Restatements and Management Turnover

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ABSTRACT: In this paper we investigate the reputational penalties to managers of firms announcing earnings restatements. More specifically, we examine management turnover and the subsequent employment of displaced managers at firms announcing earnings restatements during 1997 or 1998. In contrast to prior research (Beneish 1999; Agrawal et al. 1999), which does not find increased turnover following GAAP violations or revelation of corporate fraud, we find that 60 percent of restating firms experience a turnover of at least one top manager within 24 months of the restatement compared to 35 percent among age-, size-, and industry-matched firms. Moreover, the subsequent employment prospects of the displaced managers of restatement firms are poorer than those of the displaced managers of control firms. Our results hold after controlling for firm performance, bankruptcy, and other determinants of management turnover, and suggest that both corporate boards and the external labor market impose significant penalties on managers for violating GAAP. Also, in light of resource constraints at the SEC, our findings are encouraging as they suggest that private penalties for GAAP violations are severe and may serve as partial substitutes for public enforcement of GAAP violations.

Keywords: *restatements; reputational penalty; management turnover; aggressive accounting.*

Data Availability: *The data used in this study are available from public sources identified in the text.*

We thank two anonymous referees, James Brickley, Joe Carcello, Michelle Diaz, Patricia Dechow (editor), Neil Fargher, Kathleen Farrell, Zhaoyang Gu, Doug Hanna, Prem Jain, Simi Kedia, Mary Lea McAnally, Jody Magliolo, Paul Simko, Lynn Rees, Greg Sommers, Rex Thompson, Brett Trueman, Catherine Usoff, Kumar Venkataraman, Michael Vetsuypens, David Williams, Joanna Wu, the participants at the 2004 AAA Annual Meeting and 2004 FMA meeting for comments and discussions, and James John, Janet McDonald, Jenna Moscovic, Hari Pothineni, and Kun Wang for research assistance.

Editor's note: This paper was accepted by Patricia Dechow, Editor.

Submitted November 2003
Accepted June 2005

I. INTRODUCTION

In recent years the incidence of earnings restatements and financial reporting fraud has increased dramatically (GAO-03-138, U.S. Government Accounting Office [GAO] 2002a; Wu 2002). The erosion in the quality of earnings and financial reporting as evidenced by the proliferation of earnings restatements has caused concern among academics, practitioners, and regulators alike. Of particular concern is the extent to which managers' actions can be adequately monitored, given (1) the widely held belief that internal control mechanisms do not discipline firm managers effectively (Jensen 1993) and (2) the fact that the SEC has significant resource limitations that prohibit it from adequately investigating all alleged GAAP violations (GAO-02-302, GAO 2002b).[1] In short, these factors in combination with the dearth of evidence from prior research about adverse consequences to managers for violating GAAP (Beneish 1999) and committing other types of corporate fraud (Agrawal et al. 1999) have led to a popular perception that managers often "get away" with earnings manipulation (Abelson 1996).

In this paper, we investigate the reputational penalties to the managers of firms that restated their earnings in 1997 or 1998. Our measure of reputational penalty is management turnover and subsequent *ex post* settling up in the managerial labor market. Such an analysis is important because high management turnover coupled with a low rehire rate could influence managerial actions and incentives, *ex ante*. That is, if the managerial labor market imposes significant costs on the displaced managers in the form of a loss in income, power, prestige, and/or reputation following a restatement, then such an *ex post* settling up can provide incentives for managers to avoid manipulating earnings. Stated differently, the penalties implicit in the managerial labor market may help to reduce the regulatory costs associated with monitoring and enforcing strict adherence to GAAP.

While previous research has provided strong evidence on the incentives that exist for firms to engage in earnings manipulation (e.g., Kedia 2003; Erickson et al. 2003; Richardson et al. 2003; Beneish 1999; Dechow et al. 1996), the evidence on the consequences of earnings manipulation for managers is weak. In a sample of 64 GAAP violators that were targeted by the SEC for enforcement actions between 1987 and 1993, Beneish (1999) does not find a significant difference in the managerial turnover rate between sample firms and size-, age-, and industry-matched control firms. Similarly, Agrawal et al.'s (1999) investigation of the consequences of corporate fraud (which includes financial reporting fraud) does not document a significant association between fraud and subsequent managerial turnover over the period 1981–1992. Findings such as these contribute to the belief that management does not suffer consequences following the revelation of fraud; that is, the monitoring mechanisms do not impose sufficient discipline on managers.

In this paper, we re-examine the consequences to managers for committing GAAP violations. Our analysis is important for at least two reasons. First, there were significant changes in both internal and external monitoring mechanisms in the U.S. during the 1990s (Holmstrom and Kaplan 2001). For example, the influence of institutional owners increased dramatically, as their ownership of U.S. companies nearly doubled from 1980 to 1996 (Gompers and Metric 2001). There was also a large increase in shareholder activism in the 1990s (Gillan and Starks 2000), along with a trend toward smaller, more independent corporate boards (Yermack 1996). Consistent with these changes, Huson et al. (2001) show that the frequency of forced turnovers has increased during the 1971–1994 period and was highest over the 1989–1994 period. Similarly, a recent Booz Allen Hamilton study suggests that this trend has continued, as the rate of CEO dismissals in the largest 2,500 public companies increased by 170 percent from 1995 to 2003 (Lucier et al. 2004). Although the data point to an increase in the frequency of forced turnovers in the 1990s, the empirical evidence on the relation between performance and forced turnover has remained relatively stable over time (Huson et al. 2001). In spite of this fact, it is difficult to ignore the significant changes in the internal and external monitoring mechanisms that have occurred over the last few years. Given that our sample covers a later time period (1997–98) relative to prior studies, we are able to test whether the recent increased focus on corporate governance results in boards being more likely to punish top management when aggressive accounting or fraud is revealed.

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[1] The report issued by the GAO (GAO 2002b) finds that the SEC's workload as measured by open and pending cases has increased 65 percent and 77 percent, respectively, from 1991 to 2000, while the staff-years dedicated to these investigations have increased only 16 percent. Because the SEC is unable to address every violation, it prioritizes the cases it will pursue (Feroz et al. 1991).

The second reason that our study is important relates to the power of tests in previous research. Prior studies have provided evidence consistent with *ex post* settling up in the labor market for directors of firms committing GAAP violations (Gerety and Lehn 1997; Srinivasan 2005) with no corresponding evidence for top managers of firms in similar situations. The fact that there seem to be significant consequences for individuals who are only tangentially involved in a firm's daily operations (i.e., directors) but no such consequences for those who are directly responsible for whatever GAAP violations arise (i.e., top managers) is somewhat surprising. One potential reason for the lack of evidence is that the samples employed in management turnover studies tend to be rather small. For example, although the initial sample of Agrawal et al. (1999) consists of 103 firms, data restrictions reduce their usable sample to between 50 and 74. Moreover, given that they examine a general set of corporate fraud, their sample of accounting fraud incidents is even smaller. Similarly, Beneish's (1999) sample size is 64 firms, of which 29 firms file for bankruptcy within four years of the issuance of an Accounting and Auditing Enforcement Release (AAER). Thus, his sample size for the analysis of employment losses for nonbankrupt firms is only 35, limiting the inferences that may be drawn from his findings. This is not to say that previous research on the relationship between accounting problems and managerial turnover is flawed. However, because we are able to investigate this relationship in a period from which many more observations can be drawn (due to the increasing number of accounting violations over time), our tests are more powerful than those employed by previous researchers.

In contrast to Beneish (1999) and Agrawal et al. (1999), our results show that earnings restatements are very costly for the managers of restating firms. We examine a sample of 146 firms that announced restatements in 1997 or 1998.[2] We end our sample in 1998 to permit a sufficiently long window to track both the turnover and the subsequent employment. We find that at least one senior manager (Chairman, CEO, or President) loses his/her job within 24 months of the announcement of the restatement in 60 percent of the firms. The corresponding rate of turnover among industry-, size-, and age-matched control firms is 35 percent. The significant difference in turnover persists even after controlling for other factors associated with management turnover, such as performance, bankruptcy, and governance characteristics. Moreover, our analysis shows that the prospects of subsequent employment are significantly poorer for displaced managers of sample firms relative to their displaced counterparts at control firms. Even among managers who get rehired at one of the top three positions at a public firm, our results suggest that in contrast to the control firm managers, the sample firm managers suffer deterioration in the quality of new employment relative to their previous employment. Given that the mean age of managers in our sample is less than 50, these results suggest that, on average, managers of restatement firms suffer significant losses in reputation and very likely personal wealth.

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[2] Our sample period precedes Enron's and other high-profile restatement announcements. Thus, the reputational penalties in the time period we examine may be understated relative to the recent time period as anecdotal evidence seems to indicate that Boards are under greater pressure to respond to aggressive accounting behavior.

The fact that there are significant negative personal consequences for failing to adhere to GAAP or for aggressive interpretation of GAAP mitigates some of the widespread concern that insufficient disciplinary mechanisms have allowed managers to get away with earnings manipulation. Our findings show that once earnings manipulation is discovered, a majority of the managers face discipline from the board and from the external labor market. We maintain that an increased awareness of the penalties imposed, coupled with recent regulatory and legal actions, have the potential to influence managerial actions *ex ante*, thereby reducing the incidence of aggressive accounting or outright fraud.

The remainder of the paper is organized as follows. Section II discusses prior literature on management incentives, internal control systems, and management turnover. Section III discusses the sample of restatement firms and control firms and provides descriptive statistics on operating performance and governance characteristics prior to the restatement announcement for both groups. Section IV reports univariate and multivariate analyses of management turnover and rehire rates for restatement and control firms. Section V provides concluding remarks.

II. MANAGEMENT INCENTIVES, INTERNAL CONTROL, AND TURNOVER

Recent press reports suggest that incentive structures—in particular equity-based compensation plans including stock and option grants—that were originally viewed as a way to align managements' incentives with those of shareholders may actually prompt managers to use aggressive accounting or outright fraud to inflate stock prices (Byrne 2002). Studies have documented a greater incidence of insider selling (Beneish 1999) and stock option grants and exercises before the announcement of GAAP violations or restatements (Kedia 2003). Erickson et al. (2003) and Richardson et al. (2003) also show that stock-based compensation comprises a much larger fraction of CEO pay at restating firms relative to control firms. Murphy (1999) provides evidence that the use of stock option compensation has increased dramatically during the 1990s, suggesting that incentives to misstate earnings have increased (see also, Efendi et al. 2004; Bartov and Mohanram 2005). In addition to personal incentives, capital market incentives such as the need to maintain positive earnings surprises and the need for external financing are also greater for firms restating their earnings and/or violating GAAP (Richardson et al. 2003; Dechow et al. 1996).

What seems to have been lacking at firms such as these is an effective system of internal control, including corporate governance mechanisms designed to detect fraud, curtail aggressive accounting, and discipline managers who engage in such activities. However, it is not obvious that the optimal level of earnings management or even fraud necessarily be zero nor that every revelation of earnings management or fraud should be followed by management turnover. It is extremely costly both for a firm to design an internal control mechanism that eliminates such behavior and to replace a top manager (Agrawal et al. 1999). If, however, the revelation of fraud or aggressive accounting results in a large decline in firm value (say, due to a large penalty imposed by the capital market), then it may benefit the firm to effect the change. This would explain why boards are prone to replacing managers following poor performance or financial distress (Coughlan and Schmidt 1985; Warner et al. 1988; Weisbach 1988; Gilson 1989; Gilson and Vetsuypens 1993). Also, a large decline in market value upon the announcement of the restatement suggests that the market is imposing significant reputational penalties on the firm (Jarrell and Peltzman 1985). In such circumstances, it may be optimal to effect a managerial change to restore investors' faith in the firm and try to recover the firm's reputation.

Given the large negative share price reaction to announcements of restatements documented in recent studies, one might expect to observe a high turnover rate following the revelation of aggressive accounting or fraud, at least in cases where the GAAP violation was severe.[3] However, as stated earlier, prior evidence of management turnover following GAAP violations is weak (Beneish 1999; Agrawal et al. 1999). One exception to these findings is a study by Gerety and Lehn (1997) over an earlier time period showing that *directors* of firms charged by the SEC for accounting-related violations incur significant reputational penalties. In a sample of 62 firms charged by the SEC over the period 1981–1987, Gerety and Lehn (1997) show that following the SEC charges, there was a significant decline in the number of outside board seats held by the directors of the offending firms relative to size- and industry-matched control firms.[4]

The fact that prior studies on management turnover have not found a high turnover rate associated with the revelation of GAAP violations is somewhat surprising. However, the lack of results to date may be due to the fact that heightened shareholder activism and a strong focus on corporate governance are relatively recent trends. Holmstrom and Kaplan (2001) suggest that in the 1980s, an active takeover market played an important role in disciplining poor performers as the internal control mechanisms at U.S. firms were relatively weak. With the decline of the disciplinary takeover market, alternative governance mechanisms evolved during the 1990s to offset the decline in the active takeover market. For example, the evidence summarized in Holmstrom and Kaplan (2001) suggests that there have been significant improvements in the internal control mechanisms of U.S. firms. Moreover, increased shareholder activism and closer monitoring by institutional shareholders have improved external controls as well. It is plausible that such changes may impact a firm's propensity to discipline managers following the revelation of a GAAP violation. Another possibility for the lack of evidence on significant adverse consequences to managers of firms with GAAP violations could be that prior studies potentially lacked the statistical power necessary to detect any such consequences.

Overall, the evidence suggests that there were significant positive changes made to both internal and external monitoring mechanisms during the 1990s. However, incentives to manage earnings also increased significantly during this time period, resulting in the continued existence of aggressive accounting procedures (in spite of the increased emphasis on governance and controls). We do expect that the recent improvements in corporate governance might result in an increased propensity for boards to discipline managers in the late 1990s, relative to the time periods examined in prior studies. This notion forms the basis for our study.

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[3] Feroz et al. (1991) report a mean two-day announcement period abnormal return of –13 percent for a sample of 58 firms that were targeted by the SEC for enforcement action. Palmrose et al. (2004) report a mean two-day announcement period abnormal return of –9.2 percent for a sample of 403 restatements announcements from 1995 to 1999. Similar results are reported by Anderson and Yohn (2002), Richardson et al. (2003), Wu (2002), and Hribar and Jenkins (2004).

[4] Using a recent sample of earnings restatement firms (1997–2000), Srinivasan (2005) finds that board turnover increases significantly following restatements and that board members experience a significant loss in outside directorships.

III. DATA AND VARIABLE DEFINITIONS

Selection of Sample and Control firms

Our sample is obtained from the report submitted by the U.S. General Accounting Office (GAO) to the Chairman of the United States Senate Committee on Banking, Housing and Urban Affairs in October 2002 (GAO-03-138, GAO 2002a). The report focuses on earnings restatements and identifies 845 firms (919 announcements) that have restated their earnings from January 1, 1997 to June 30, 2002. The sample was constructed from a search of Lexis-Nexis using variations of the word "restate" and identifies firms that announced nonroutine, material restatements of previously issued financial statements. The report identifies events involving "accounting irregularities, including so-called 'aggressive' accounting practices, intentional and unintentional misuse of facts applied to financial statements, oversight or misinterpretation of accounting rules, and fraud." The report also identifies the reason for the restatements (e.g., revenue recognition or restatement of previously recorded In-Process R&D [IPR&D] charges) as well as the party that prompted the restatement, if known (i.e., the firm, auditor, or the SEC).

Our initial sample consists of 194 events announced by 187 firms identified by the General Accounting Office (GAO) for the years 1997 and 1998. Because our primary interest lies in documenting managerial turnover and the subsequent hiring of the displaced managers in comparable positions at other firms, we end our sample in 1998 to permit a sufficiently long post-restatement period to track the managers who have been displaced. As detailed in Panel A of Table 1, from the initial sample of 194 we eliminated all firms that are not on the CRSP database as well as firms that do not have a CRSP share code of 10 or 11 (typically foreign firms). This reduced the sample to 169 restatement observations.

To examine reputational penalties, it would be helpful to separate forced departures from voluntary departures; however, this is often difficult to determine due to the nature of the news articles, if any, announcing the departure.[5] Thus, in order to provide a benchmark for the management turnover analysis, we selected a control firm for each sample firm based on industry, age, and size. Prior research suggests that the SEC's decision to scrutinize a firm's financial statements is based on its age; that is, the SEC is more likely to scrutinize young growth firms (Feroz et al. 1991; Beasley 1996; Beneish 1997, 1999). Also, young firms are more likely to be in need of external financing, which may give them an added motivation to manipulate earnings. Requiring that an industry-, age-, and size-matched control firm be available for each sample firm reduced our final sample to 146 sample firms and 146 control firms (details are reported in Panel A of Table 1).

Management Turnover

We identified the managers of the restatement and control firms pre- and post-restatement from the proxy statements issued by the firms. If the proxy was not available, then we searched 10-Ks and 8-Ks. We define management turnover as the turnover of a person or persons holding titles of Chairman, CEO, and/or President. A firm is said to have

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[5] This is especially true for our sample as the firms are quite small and, as a result, news coverage is not detailed enough to ascertain whether some of these departures are voluntary. Nonetheless, we did try to identify circumstances surrounding the departure of the managers. For the sample firms (112 departures), 59 managers either resigned or were terminated (10) by the board. Eleven of the managers apparently retired and one manager departed due to a merger. For 43 departures, there was no specific mention of the reason for the departure. For the control firms (63 departures), seven managers resigned (no terminations), three retired, one left due to a merger, and for the remaining 52 departures there was no specific reason that was given. We repeated the analysis in Table 3 after removing the 14 turnovers that were retirements (11 for sample firms and 3 for control firms), and the results are similar.

TABLE 1
Composition of Sample

Panel A: Restrictions Leading to Final Sample

Restriction	**Sample Firms**
1. Initial Sample	194
2. Firms on CRSP	180
3. Firms having CRSP share codes of 10 or 11	169
4. Difference between sample firm and control firm age is one year or less and ratio of sample firm to control firm market capitalization is between 75 percent and 125 percent	125
5. Difference between sample firm and control firm age is two years or less and ratio of sample firm to control firm market capitalization is between 50 percent and 150 percent (Relaxed Restriction 4)	151
6. Sufficient post-restatement data available for turnover analysis for both control and sample firm	146
Final Sample	**146**

Panel B: Industry Composition

Two-Digit SIC Code	**Turnover Firms**[a]	**No Turnover Firms**	**Total**
28—Chemicals	1	6	7
35—Industrial Machinery	7	6	13
36—Electrical Equipment	10	7	17
38—Measuring Instruments	7	2	9
48—Communications	5	2	7
67—Investment Offices	3	5	8
73—Business Services	24	13	37
80—Health Services	5	1	6
All Others (<5 per industry)	25	17	42
Total	87	59	146

[a] Turnover firms are those that experience turnover in at least one of the three positions of Chairman, CEO, or President within 24 months of the restatement announcement.

turnover if an individual(s) holding the title of either Chairman, CEO, or President leaves the company within 24 months of the restatement announcement.[6] Our classification of turnover is conservative for a number of reasons. If the manager is of age 65 or above at the time of restatement and leaves the company within 24 months of restatement, then we do not classify that firm as a turnover case.[7] Also, if the company merges or is acquired within 24 months and as long as the manager does not leave the company prior to the merger, we do not consider it as a turnover, even though prior research suggests that an acquisition significantly increases the likelihood of managerial turnover (Agrawal and Walkling 1994; Martin and McConnell 1991). In addition, we use a shorter window of 24 months following the restatement as opposed to the four-year period used by Beneish (1999) for two reasons. First, we believe it is difficult to attribute turnover to an earnings restatement if the turnover does not occur within a relatively short period following the restatement announcement. Second, firms that have restated earnings perform poorly following the restatement, on average, and prior evidence suggests that poor performance leads to management turnover. A shorter window therefore reduces the confounding effect of performance on turnover (of course, we control for pre- and post-restatement performance in our regressions). Finally, it is possible that a turnover may occur prior to the first announcement of any possible restatement, but actually be related to the eventual restatement. Such cases, if they exist, would understate the turnover rate associated with the restatements in our study.

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[6] Thus, if a person occupied the position of Chairman and CEO prior to the restatement but following the restatement retains the title of only Chairman, this is not considered a turnover. We replicate the analysis using an alternative definition where we focus on turnover in positions (not people) and find similar results.

[7] To ensure that our findings are not sensitive to this age cut-off, we replicated the main results using an alternative age cut-off of 60. Using this alternative age cut-off, we find that 53 percent of the sample firms experience a turnover, compared to only 31 percent of the control firms. In untabulated regressions that replicate the three models presented in Panel B of Table 3 with the age 60 cut-off, the p-value for *RESTATE* is consistently below 0.05. Thus, our results and inferences are robust to this alternative specification.

Summary Statistics

In Panel B of Table 1 and in Table 2 we report various summary statistics for the sample and control firms. Panel B of Table 1 details the industry composition of the restatement firms. The industry that is most heavily represented (25 percent of sample observations) is Business Services. Restatements are also relatively common among firms involved in the production of electrical equipment and industrial machinery, with 12 percent and 9 percent of the sample, respectively, coming from these two industries.

In Table 2 we present a variety of performance- and governance-related measures for our sample and control firms. All of the performance and governance variables are measured in the year prior to the announcement of the restatement. Similar to Kinney and McDaniel (1989), we find that restatement firms are smaller, less profitable, and more leveraged than their industry peers. The mean market value of equity of the firms in the restatement sample is $871.41 million, which corresponds to a mean size decile rank of 2.75, based on the universe of NYSE firms. The mean (median) age of the restatement firms is 91.53 (49) months. As expected given our selection criteria, the control firms match the sample firms closely on size and age.

Operating performance statistics indicate that sample firms perform poorly in the year prior to the announcement of the restatement, relative to the control firms. The median ROA in the year prior to the announcement is 3.2 percent versus 12.1 percent for the control firms. Stock market performance of the sample firms is also poor prior to the announcement of the restatement. Average market-adjusted returns from month −12 to −1 relative to the announcement month are −25 percent. This is consistent with Beneish's (1999) observation of negative stock market performance prior to revelation of a GAAP violation. The restatement firms continue to perform poorly after the restatement, on average, as indicated by market-adjusted returns of −19.2 percent in months +1 to +12.

We also examine the accruals of the sample firms relative to those of the control firms, initially measured in year −1 relative to the restatement announcement. Following Richardson et al. (2003), we examine two measures of accruals: operating accruals, defined as earnings minus cash flow from operations (earnings − CFO); and total accruals, defined as earnings − CFO − cash flow from investing accruals (CFI). The mean (median) operating accruals-to-average assets ratio for the sample firms is −9.2 percent (−6.8 percent). The corresponding mean (median) for the control firms is −3.1 percent (−3.6 percent). Both differences are marginally significant. Total accruals are not significantly different across the sample and the control firms. When we measure accruals in the fiscal

TABLE 2
Selected Summary Statistics for Sample and Control Firms

Panel A: Financial Data

		Restatement Firms		Control Firms			Wilcoxon
Variable	**n**	**Mean**	**Median**	**Mean**	**Median**	**t-stat p-value for Difference**	**p-value for Difference**
Market Value of Equity	146	871.407	98.034	714.770	89.803	0.607	0.764
FF MVE Decile Rank	146	2.747	1.000	2.637	1.000	0.695	0.804
Firm Age (months)	146	91.534	49.000	86.116	43.500	0.722	0.851
Book-to-Market Ratio	94	0.505	0.337	0.585	0.479	0.403	0.019
FF BTM Decile Rank	94	4.021	3.000	4.984	5.000	0.030	0.019
Debt/Assets (yr–1)	113	0.240	0.201	0.195	0.144	0.109	0.074
Total Assets (yr–1)	114	1189.701	89.024	984.257	100.840	0.696	0.639
Return on Assets (yr–1)	107	−0.055	0.032	0.027	0.121	0.062	0.004
Raw Return, months −12 to −1	144	−0.115	−0.245	0.109	0.070	0.002	0.001
Mkt-Adj Return, months −12 to −1	144	−0.253	−0.361	−0.029	−0.034	0.001	0.001
Raw Return, months +1 to +12	136	−0.095	−0.235	0.045	−0.100	0.110	0.079
Mkt-Adj Return, months +1 to +12	136	−0.192	−0.307	−0.061	−0.136	0.117	0.060
Operating Accruals/Assets (yr–1)	103	−0.092	−0.068	−0.031	−0.036	0.057	0.077
Total Accruals/Assets (yr–1)	103	0.071	0.052	0.076	0.032	0.899	0.786

Panel B: Governance Data

Age of CEO	125	48.8	49.0	50.4	51.0	0.134	0.159
Tenure of CEO	129	6.5	4.0	6.6	5.0	0.967	0.484
Age of Chairman	112	51.7	52.0	53.1	53.0	0.337	0.293
Tenure of Chairman	116	6.9	5.0	7.4	5.5	0.499	0.380
Age of President	118	48.3	49.0	48.8	49.0	0.639	0.592
Tenure of President	122	5.8	4.0	5.3	4.0	0.491	0.912
Chm/CEO	140	60.7	1.0	62.1	1.0	0.806	0.807
% Insider Ownership	113	26.6	19.5	24.5	17.2	0.498	0.918
5% and Greater Blockholders	112	18.0	14.3	22.1	17.9	0.117	0.123
% Outside Directors on the Board	112	60.7	62.5	71.1	75.0	0.001	0.001

(*continued on next page*)

TABLE 2 (Continued)

The table reports various summary statistics for the sample of restatement firms and their age-, size-, and industry-matched control firms. All accounting data are taken from the fiscal year-end immediately prior to the restatement announcement.

Market value of equity is calculated as of 20 trading days preceding the restatement announcement; Decile breakpoints for size and book-to-market ratio are based on the universe of NYSE/AMEX firms; Firm Age is calculated as the number of months from the CRSP BEGDAT to the restatement announcement; Book-to-Market ratio is calculated as book value of equity, excluding negative book value firms (Compustat item 60) divided by market value of equity as of the year-end preceding the restatement announcement; Debt/Assets = (Compustat Item 9 + Compustat Item 34)/Compustat Item 6; Total assets (Compustat item 6) are as of the fiscal year-end immediately preceding the restatement; Return on Assets = Compustat Item 13/[(Compustat Item 6 in year t + Compustat Item 6 in year $t-1$)/2]; Raw returns, -12 to -1 are buy-and-hold returns over months -12 to -1 relative to the restatement announcement; Market-adjusted returns, -12 to -1 are relative to the CRSP equally weighted index; Operating Accruals/Assets = (Compustat Item 123—Compustat Item 308)/[(Compustat Item 6 in year t + Compustat Item 6 in year $t-1$)/2]; Total Accruals/Assets = (Compustat Item 123—Compustat Item 308 − Compustat Item 311)/[(Compustat Item 6 in year t + Compustat Item 6 in year $t-1$)/2]; Age and Tenure variables are taken from the proxy statement preceding the restatement announcement; Chm/CEO = 1 if the same person holds the title of Chairman and CEO, and 0 otherwise; Insider ownership, blockholder ownership, and board composition data are taken from proxy statements in the year prior to the restatement announcement.

All nonemployee directors are considered outside directors.

The varying number of observations is due to missing data for either sample or control firms. We require both the sample firm and the control firm to have data for each line item, such that only complete pairs are included in the analysis.

year that is eventually restated, the differences are not significant. These results are not completely surprising, however, given that the restatement firms are very poor performers prior to the restatement.

Turning to the governance-related statistics reported in Panel B of Table 2, we find that the average age of the CEO among sample (control) firms is 48.8 (50.4) years and the average CEO tenure is 6.5 (6.6) years, both of which are not significantly different. The sample and control firms also match very closely on the age and tenure characteristics of the President and Chairman. The incidence of CEOs also holding the title of Chairman is similar across sample and control firms (61 percent of the sample firms and 62 percent of the control firms have the same person holding both titles), suggesting that the extent of entrenchment of top management does not appear to be different across sample and control firms. Neither percentage of shares held by insiders nor blockholder ownership is significantly different across sample and control firms. Sample firms do, however, have significantly less independent boards, with only 60.7 percent outside directors as compared to 71.1 percent for control firms. This result is similar to Farber (2005), but different from Agrawal and Chadha (2002). Overall, the governance statistics suggest that the sample and control firms match closely on most dimensions, with the exception of the proportion of outside directors.

IV. RESULTS

Market Reaction to the Announcement of Restatements

The market's perception of the severity of the restatement could be an important factor for the corporate board in determining whether management should be disciplined. To test this perception, we calculate (but do not tabulate) abnormal returns for the sample of restatement and control firms having sufficient returns data during the announcement period (day −1 to day +1). Our discussion of these results is brief as several other papers have examined announcement period returns in detail (e.g., Palmrose et al. 2004; Wu 2002; Anderson and Yohn 2002). The mean three-day market-adjusted (CRSP equal-weighted index) abnormal return for our sample of restatement firms is −11.07 percent, which is similar in magnitude to returns documented in prior research on earnings restatements. The magnitude of the price response suggests that the market is surprised by the announcement, that the restatement represents a significant event for the company, and that the market imposes a significant penalty on firms committing GAAP violations.

Also consistent with prior studies, restatements resulting from improper revenue recognition are the most common (37 percent) and generate the largest negative reaction (−14.89 percent). The announcement-period reaction to restatements resulting from improper cost recognition is also large, at −10.51 percent. Restatements resulting from all other reasons, including improper valuation and allocation of amounts of In-Process R&D, improper asset write-offs, inventory valuation, restructuring charges, and others generate an abnormal return of −7.98 percent. When we partition based on the party initiating the restatement, we find the average reaction to the 62 company-prompted restatements is −11.33 percent, while the average reaction to the 22 auditor-prompted restatements is the strongest at −15.21 percent. The average abnormal return associated with all other prompters, including those that were not identified, is −9.34 percent.

Management Turnover Analysis

Rates of Management Turnover

Panel A of Table 3 reports the incidence of management turnover among sample and control firms. In 87 out of 146 sample firms (59.6 percent), at least one person holding the

TABLE 3
Management Turnover Analysis

Panel A: Management Turnover for Sample and Control Firms

Partition	n	Sample Firms with Turnover	Control Firms with Turnover	χ^2 Statistic	p-value
All Pairs	146	87 (59.6%)	51 (34.9%)	17.81	<0.01
Excluding Bankrupt Pairs	127	74 (58.3%)	43 (33.9%)	15.23	<0.01
Excluding AAER Pairs	111	57 (51.4%)	39 (35.1%)	5.95	0.01
Excluding Bankrupt Pairs and AAER Pairs	97	49 (50.5%)	35 (36.1%)	4.12	0.04

Panel B: Logistic Regression—Dependent Variable is Management Turnover (0,1)

Variable	Coeff	p-value	Coeff	p-value	Coeff	p-value
Intercept	−0.08	0.73	−0.26	0.37	0.50	0.28
RESTATE	0.73	<0.01	0.66	0.04	0.76	0.03
CHM/CEO	−0.79	<0.01	−0.74	0.02	−0.84	0.02
STKPERF1	−0.57	0.02	−0.63	0.02	−0.47	0.12
STKPERF2	−0.53	<0.01	−0.41	0.09	−0.42	0.11
ROA	—	—	−1.60	<0.01	−1.25	0.02
BLKOWN	—	—	—	—	−0.02	0.09
INSOWN	—	—	—	—	−0.02	0.03
Pseudo R^2	0.096		0.126		0.132	
# of Observations	268		202		170	

Panel C: Logistic Regression—Dependent Variable is Management Turnover (0,1) for CEO Turnover *Only*

Intercept	2.35	0.08	2.07	0.14	3.76	0.04
RESTATE	1.03	<0.01	0.97	0.02	1.20	0.02
CHM/CEO	−0.88	0.02	−0.91	0.04	−1.26	0.03
STKPERF1	−0.82	0.02	−0.68	0.09	−0.45	0.33
STKPERF2	−0.71	0.02	−0.96	0.02	−0.70	0.13
CEOAGE	−0.05	0.07	−0.05	0.05	−0.08	0.03
FIRMSIZE	−0.19	0.40	−0.09	0.50	−0.20	0.11
*CEOAGE*FIRMSIZE*	<0.01	0.40	<0.01	0.51	<0.01	0.09
TENURE	−0.38	0.12	−0.07	0.83	0.09	0.82
ROA	—	—	−1.35	0.03	−1.36	0.04
BLKOWN	—	—	—	—	−0.02	0.12
INSOWN	—	—	—	—	−0.02	0.12
Pseudo R^2	0.201		0.219		0.229	
# of Observations	188		146		122	

(*continued on next page*)

TABLE 3 (Continued)

Panel A reports management turnover for the sample and control firms over the period of 24 months following the restatement announcement. Management turnover is defined as the change in any individual holding the title of Chairman, CEO, and/or President within 24 months of the restatement. The control firms are selected based on age, size, and industry.

Panels B and C variable definitions are as follows:

RESTATE = 1 if the firm is a restatement firm, and 0 if the firm is a control firm;
CHM/CEO = 1 if the same person held the title of both Chairman and CEO, and 0 otherwise;
STKPERF1 (*STKPERF2*) = raw buy-and-hold returns over months −12 to −1 (+1 to +12) relative to the restatement announcement;
CEOAGE = age of CEO at time of the restatement;
FIRMSIZE = market value of equity one month prior to restatement;
TENURE = 0, 1, or 2 if the number of years as officer prior to turnover is in the bottom quartile, between the bottom and top quartile, or in the top quartile of the distribution, respectively;
ROA = Return on Assets, as defined in Table 2 and is for the fiscal year prior to the restatement announcement;
BLKOWN = the 5 percent and greater blockholder percentage prior to the restatement announcement; and
INSOWN = the insider ownership percentage prior to the restatement announcement.

We require the sample firm and the control firm to have data for each variable, such that only complete pairs are included in the analysis.

title of either Chairman, CEO, or President left the company within 24 months of the announcement of the restatement. Over the same time period the corresponding turnover for the control firms is 51 (34.9 percent). This difference in turnover between sample and control firms is statistically significant (p-value < 0.01). Thus, managers of restating firms are significantly more likely to lose their jobs than their counterparts at control firms that did not restate their earnings. In the 87 restatement firms that experienced a turnover, 112 individuals were displaced (an average of 1.29 individuals per firm for those firms experiencing turnover). The corresponding number of individuals displaced at the 51 control firms experiencing turnover is 63 (an average of 1.24 per turnover firm).

Sensitivity of Findings to Bankruptcies and AAERs

In this section we investigate the sensitivity of our results to removing the firms that filed for bankruptcy or that were the subject of Accounting and Auditing Enforcement Releases (AAERs). Prior studies have shown significantly higher management turnover for bankrupt firms as compared to firms that are not facing bankruptcy (Beneish 1999; Gilson and Vetsuypens 1993; Gilson 1989). In our study, there is a greater frequency of bankruptcy filings for sample firms as compared to the control firms (15 out of 146 sample firms filed for bankruptcy within 24 months of the announcement of the restatement, whereas only 4 control firms filed for bankruptcy). To abstract away from the bankruptcy effect, we replicate the turnover analysis in Panel A of Table 3 after removing the 19 pairs where either the sample or control firm filed for bankruptcy. The results of the turnover analysis for nonbankrupt pairs, also presented in Panel A of Table 3, are similar to the full sample results. Specifically, the number of nonbankrupt sample firms experiencing turnover is 74 out of 127 (58.3 percent), while the corresponding number for control firms is only 43 out of 127 (33.9 percent). The difference between sample and control firm turnover is again significant at the 1 percent level.

We also investigate the frequency with which sample and control firms were the subjects of AAERs during our sample period, as executives are often barred from serving as officers of public companies as the result of an enforcement release. We gather data on

AAERs from the list of Litigation Releases and Administrative Proceedings at the SEC's website (http://www.sec.gov) and find that 34 sample firms and one control firm were the subjects of enforcement actions between 1997 and 2002.[8] Panel A of Table 3 shows that even after these 35 pairs are removed from the analysis, the rate of management turnover among sample firms is still significantly higher (p-value = 0.01) than that of control firms.[9] Furthermore, when we remove the union of all bankrupt firm and AAER firm pairs, a significant difference (p-value = 0.04) remains between the turnover rates of sample and control firms.

Logistic Regression Analysis

The univariate comparisons presented in Panel A of Table 3 show that the rate of management turnover at the sample firms is significantly higher than the rate of turnover at control firms. However, given that various other factors also affect the likelihood of management turnover, we now relate the restatement to the likelihood of turnover in a multivariate regression framework. We also present, in a later section, the results of a differences-in-differences design where we include turnover for sample and control firms prior to the restatement announcements to ensure that unspecified firm-specific differences are not driving the results.

Prior literature has shown that poor performance increases the likelihood of managerial turnover (Warner et al. 1988; and many others). We use the firm's stock return prior to the announcement of the restatement (month −12 to −1) as our measure of prior performance (*STKPERF1*). We also use the firm's *ROA* in the year preceding the announcement of the restatement as an additional measure of prior performance, as Engel et al. (2003) find that both market-based and accounting-based measures of performance are important in explaining CEO turnover.[10] Also, because we examine management turnover in the two years following the announcement of the restatement, it is possible that post-restatement performance may influence turnover as well. Thus, we control for post-restatement performance (*STKPERF2*) using a firm's stock return over the 12 months following the announcement of the restatement (month +1 to +12).

In addition to the performance-based controls, we also control for managerial entrenchment using two proxies. Our first proxy for entrenchment defines cases where the same person holds the titles of CEO and Chairman (*CHM/CEO* = 1). Prior research (Beasley 1996; Dechow et al. 1996; among others) has used this variable as a measure of the CEO's influence on the board.[11] The second proxy is the level of insider ownership (*INSOWN*). Finally, firms with higher levels of block-ownership are likely to be better monitored (Shleifer and Vishny 1986), so we include the level of 5 percent block ownership (*BLKOWN*) as a control variable in the regression analysis.[12]

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[8] The relatively low frequency of AAERs for our sample firms suggests that the SEC is indeed unable or unwilling to go after every violator, as suggested by Feroz et al. (1991). Moreover, there is a significant time lag between the announcement of the restatement and the issuance of an AAER. In our sample, we find that the mean (median) lag between the restatement announcement and the issuance of the first AAER is 35 (33) months.

[9] Out of the 34 sample firms that were the subject of an AAER, 30 of these have turnover in at least one of the top three positions (i.e., coded as a turnover firm in this study). Thus, 30 of the 87 (34 percent) restatement firms experiencing turnover were the subject of an AAER, yet we still find a significant difference in turnover after removing these observations.

[10] We also estimate the logistic regression model including leverage (the debt-to-assets ratio in Table 2), given that there is a marginally significant difference for this variable between sample and control firms. Leverage is not significant in the model and the other results in Table 3 remained unchanged. Similarly, when we replace ROA with the one-year change in ROA, the coefficient for change in ROA is insignificant but the other coefficients remain statistically significant.

We estimate a logistic regression model where the dependent variable equals 1 if the firm experienced turnover in at least one of the top three positions within 24 months of the restatement announcement. The results of our initial model are reported in Panel B of Table 3. We first discuss the results without including *ROA* and the ownership control variables because we lose a number of observations in these models due to missing accounting and ownership data. The pseudo-R^2 of the first model is 9.6 percent and the percent concordant observations is 70.4 percent. A naïve model could predict 46 percent as a total of 124 out of the 268 firms had a turnover, so the model has strong predictive ability. Consistent with prior research, the results show that both pre and post stock market performance, namely *STKPERF1* and *STKPERF2*, are negatively and significantly related to the likelihood of turnover. The coefficient on the *CHM/CEO* variable is negative and significant as well (-0.79, p-value < 0.01), suggesting that the unitary structure reduces the likelihood of managerial turnover. This result is consistent with arguments in Jensen (1993) that corporate boards will be less likely to remove an individual quickly if the same individual holds the titles of CEO and Chairman. The coefficient on the restatement variable is positive (0.73) and significant (p value < 0.01), suggesting that even after controlling for performance and managerial entrenchment, restatements are positively and significantly related to managerial turnover.

When we add *ROA* to the model, we find that it is also significantly and negatively related to the likelihood of turnover.[13] At the mean values for the prior return, post return, *ROA*, and *CHM/CEO* variables, the probability of a turnover increases from 33 percent for a non-restating firm to 49 percent for a restating firm. The regression results are robust to including the block-ownership and insider ownership control variables, as can be seen in the rightmost two columns of Panel A of Table 3. The coefficient on *RESTATE* remains positive and significant. As expected, the coefficient on the inside ownership is negative and significant suggesting that, *ceteris paribus*, an insider with higher ownership is more difficult to remove. However, the coefficient on block-ownership, somewhat surprisingly is negative, though not significant.[14] In total, our findings suggest that an earnings restatement is a strong predictor of top management turnover, even after controlling for several factors that are known determinants of changes in management.[15]

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[11] An alternative interpretation of the ramifications of the same individual holding both titles is advanced by Brickley et al. (1997). They suggest that the unitary structure of having the same person holding both titles also has several benefits and is not necessarily indicative of entrenchment or high levels of agency costs. They argue that the optimal structure for the firm depends on the economic circumstances facing the firm. The sample and control firms in this study likely face a similar economic environment, given that control firms are selected based on industry, size and age.

[12] We do not include a variable for CEO age or an indicator variable for CEOs over the age 64 as in Engel et al. (2003) and Murphy and Zimmerman (1993) for two reasons. First, our dependent variable is at the firm level rather than the individual executive level. Second, to be conservative, we have coded the cases where the CEO was over age 64 and left the company within 24 months of the restatement as no turnover cases.

[13] The percent concordant observations for this model is 73.3 percent. To assess the classification rates at various cut-off points, we begin with the cut-off probability value that minimizes the sum of a type I and a type II error. At this probability value of 0.39, the percentage of turnovers correctly classified is 76.77 percent, while the percentage of nonturnovers correctly classified is 62.93 percent. The naïve model would predict 57.4 percent of nonturnovers correctly. Thus, the model has acceptable discrimination ability.

[14] Following Weisbach (1988), we also interacted block-ownership with performance variables to test if the sensitivity of performance to turnover is increasing in the level of block-ownership. None of the coefficients on the interaction variables are significant.

An Analysis of CEO Turnover

In Panel C of Table 3, we report results for models where only firms having cases of CEO turnover are classified as turnover firms. We do this for two reasons. First, focusing on just the CEO allows us to introduce the age and the tenure of the CEO as control variables in the turnover regression. Prior research suggests that CEO age is an important determinant of CEO turnover and that its importance increases with firm size (Murphy 1999). Also, a CEO with longer tenure is likely to be more difficult to replace. Second, Agrawal et al. (1999) analyze CEO turnover separately and do not find that the revelation of corporate fraud increases the likelihood of CEO turnover. Thus, a separate analysis of CEO turnover permits a comparison of our results with theirs.

We find that 51 percent of the restatement firms replaced their CEO within two years of the restatement, compared to only 23 percent of the control firms. This univariate difference is significant at the 1 percent level. Focusing on the regression results presented in Panel C of Table 3, we find that an earnings restatement significantly increases the likelihood of CEO turnover after controlling for other determinants of CEO turnover. Not surprisingly, we also find that performance is negatively related to the likelihood of turnover. CEO age is negatively related to the likelihood of turnover, consistent with findings in Murphy (1999). Murphy (1999) also finds that the importance of age in explaining turnover increases with firm size, so we also include an interaction between CEO age and firm size, although the coefficient is not significant. We include CEO tenure in the regressions as a control variable, but the coefficient, though negative, is not significant in the presence of CEO age. The relationships between the other control variables and turnover are similar to those that are reported for the full sample. Thus, overall, the results from our CEO-turnover-only analysis are very similar to the results comprising the turnover of all three top executives.

Management Turnover Using an Alternative Approach

To the extent that there are some idiosyncratic differences in restatement firms and control firms that are not controlled for in the multivariate analysis, the results may be driven by these omitted and unobserved firm-specific differences. An alternative approach that mitigates this concern is a difference-in-differences approach, where we examine the turnover rate of the sample firms and the control firms in the pre-restatement period as well as the post-restatement period. Specifically, we compute the turnover rate for all firms in the 24 months prior to the restatement date and then compare the pre-restatement turnover rate to the turnover rate over the 24 months following the restatement. With this approach, we are effectively using a firm as its own control in addition to controlling for other known factors.

Table 4 shows that in the 24 months prior to the announcement of the restatement, 31 sample firms (21 percent) experienced a turnover. Over the same period, 30 control firms (21 percent) experienced a turnover. The difference is not significant (p-value = 0.89), suggesting that prior to the restatement, both the sample and control firms had similar turnover rates. In the 24-month post-restatement period, the turnover rate for control firms

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[15] As a sensitivity test, we repeated all analyses after removing the 19 pairs of firms that filed for bankruptcy within 24 months of the restatement announcement (15 sample firms and 4 control firms filed for bankruptcy). We also removed all firms that were the subject of AAERs between 1997 and 2002. The results from the alternative models are similar and, hence, the inference from these models remains the same.

TABLE 4
Management Turnover Analysis: Pre- versus Post-Period

Panel A: Management Turnover for Sample and Control Firms

Partition	n	Sample Firms with Turnover	Control Firms with Turnover	χ^2 Statistic	p-value
Pre-Restatement Period	146	31 (21%)	30 (21%)	0.02	0.89
Post-Restatement Period	146	87 (60%)	51 (35%)	17.81	<0.01

Panel B: Logistic Regression—Dependent Variable is Management Turnover (0,1)

Variable	Coeff	p-value	Coeff	p-value	Coeff	p-value
Intercept	−0.90	<0.01	−0.41	0.23	−0.03	0.94
RESTATE	0.01	0.96	0.08	0.85	−0.29	0.54
*RESTATE*AFTER*	1.75	<0.01	1.03	<0.01	0.93	0.03
*CONTROL*AFTER*	0.73	<0.01	0.27	0.47	0.06	0.88
CHM/CEO	−0.78	<0.01	−0.87	<0.01	−0.99	<0.01
STKPERF1	—	—	0.04	0.71	−0.37	0.07
STKPERF2	—	—	−0.47	0.01	−0.45	0.03
ROA	—	—	−1.48	<0.01	−1.90	<0.01
BLKOWN	—	—	—	—	<0.01	0.56
INSOWN	—	—	—	—	<−0.01	0.75
Pseudo-R^2	0.108		0.135		0.146	
Number of Observations	584		374		324	

Panel A reports management turnover for the sample and control firms over the period of 24 months prior to the announcement of a restatement in the pre-restatement period and 24 months following the announcement of a restatement in the post-restatement period. Management turnover is defined as the change in any individual holding the title of Chairman, CEO, and/or President. The control firms are selected based on industry, age, and size.

Panel B variable definitions are as follows:

RESTATE = as defined previously;
AFTER = 1 for post-restatement periods, and 0 for pre-restatement periods;
CONTROL = 1 if the firm is a control firm, and 0 if the firm is a restatement firm;
CHM/CEO = as defined previously and is measured in year −3 for the pre-restatement period and immediately prior to the restatement for the post-restatement period;
STKPERF1 (*STKPERF2*) = the raw buy-and-hold return from month −36 to −25 (−24 to −13) for the pre-restatement period and from month −12 to −1 (+1 to +12) for the post-restatement period;
ROA = equal to ROA in year −3 for the pre-restatement period and in year −1 for the post-restatement period; and
BLKOWN (*INSOWN*) = 5 percent and greater blockholder ownership (inside ownership) in year −3 for the pre-restatement period and in the year prior to the restatement for the post-restatement period.

jumps to 35 percent; however, the turnover rate for the sample firms almost triples to 60 percent. The increases in turnover rates in the post-restatement period for both the sample firms and control firms are significant. However, the increase in the turnover rate for the sample firms is significantly greater than the increase in the turnover rate for the control firms (p-value < 0.01).

In addition to the univariate analysis we also incorporate the difference-in-differences approach in a multivariate analysis. In this analysis we use two interaction terms—

*RESTATE*AFTER* and *CONTROL*AFTER*—where *AFTER* = 1 in the post-restatement period and 0 in the pre-restatement period. We first report results excluding accounting and market performance variables as we lose observations by requiring these variables to be present in both the pre and post periods. Results for this reduced model, reported in the first two columns of Panel B of Table 4, show that the coefficients on both *RESTATE*AFTER* (1.75, p-value < 0.01) and *CONTROL*AFTER* (0.73, p-value = 0.02) are positive and statistically significant, indicating an increase in the rate of turnover for both sample and control firms following the restatement. However, the coefficient for *RESTATE*AFTER* is significantly larger (p-value < 0.01) than the coefficient for *CONTROL*AFTER*, indicating that the incidence of management turnover in the post-restatement period is higher among restatement firms than it is among control firms. The unitary structure of the same individual occupying the positions of Chairman and CEO continues to be negative and significant in this alternative specification as well.

In the second model reported in Panel B of Table 4, we control for the impact of firm performance on both pre- and post-restatement turnover using the accounting as well as the stock market measures of firm performance. Specifically, *STKPERF1* (*STKPERF2*) is measured over months −36 to −25 (−24 to −13) for the pre-restatement turnovers and over months −12 to −1 (+1 to +12) for the post-restatement turnovers. Performance measures over two years are included to control for performance during our turnover measurement window to mitigate concerns that the turnover could be due to poor performance during the turnover measurement window rather than the restatement event. We also use ROA of the firm to control for the effect of accounting performance on turnover. For the pre-restatement turnovers, ROA is measured at the end of the fiscal year ending immediately prior to month −24 relative to the restatement announcement (year −3). For the post-restatement turnovers, *ROA* is measured at the end of the fiscal year immediately prior to the announcement of the restatement (year −1). We do not include *ROA* over year −2 or year +1 as the correlation between the *ROA* measured over two consecutive years is very high.[16]

Similar to the results presented earlier in Table 3, results reported in Panel B of Table 4 show that firm performance (accounting and stock market) does significantly influence management turnover. Specifically, turnover is less likely for firms having higher levels of profitability and superior stock market performance. With respect to the influence of restatements on turnover in the presence of performance controls, *RESTATE*AFTER* is positive (1.03) and statistically significant (p-value < 0.01) indicating increased turnover in the post-restatement period. For a restatement firm, the probability of a turnover increases from 30 percent in the pre-restatement period to 55 percent in the post-restatement period (at the mean values for the prior return, post return, *ROA*, and *CHM/CEO* variables). On the other hand, *CONTROL*AFTER* is not significantly different from zero (0.27, p-value = 0.47) once we control for performance. The probability of a turnover for a control firm in the post-restatement period, again at the mean values for the prior return, post return, *ROA*, and *CHM/CEO* variable, is 28 percent as compared to the 55 percent for a restatement firm. Panel B of Table 4 shows that similar findings obtain when we include controls for ownership structure as well. Thus, even though turnover did increase for the control firms in the post-restatement period, this increase can be attributed to poor performance. Poor performance does not, however, fully explain the increased turnover for restatement firms.

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[16] Our results and inferences do not change if we include ROA over two consecutive years in the pre- and the post-period.

Further Disentangling the Impact of Performance and Restatement on Turnover

The univariate comparisons in Table 2 indicate that restatement firms have performed poorly prior to the restatement, as compared to the control firms, on average. This raises a concern that it is poor performance and not the restatement, per se, that may be driving the increased turnover following the restatement announcement. In the regressions presented in Tables 3 and 4, we have controlled for both stock market and accounting measures of firm performance preceding and during our turnover measurement windows in the pre and post periods. Also, because the window over which we measure turnover is relatively short (two years), the likelihood that performance is driving our results is reduced. This differs from Agrawal et al. (1999) and Beneish (1999) who measure turnover over longer periods—three years and four years, respectively—which increases the likelihood that post-event performance may be influencing the rate of turnover.

To further establish that the impact of a restatement on management turnover is incremental to the impact of firm performance on turnover, we partition our sample of restatement firms by the party prompting the restatement. By partitioning the sample, we can compare within the restatement firms where we might expect turnover rates to vary, but we do not, *ex ante*, expect performance to vary. If the company voluntarily acknowledges its error and prompts a restatement, then it is also probably more likely to take action against top management. In a related vein, restatements that are prompted by firms' auditors are likely to be more severe (as is evidenced by the fact that auditor-prompted restatements are associated with the most negative announcement-period returns).[17] In both situations—i.e., company- and auditor-prompted restatements—we expect turnover to be higher than in all other cases, but we do not have any reason to expect that accounting or stock market performance would vary.

The results reported in Panel A of Table 5 show that 68 percent of company- and auditor-prompted restatements result in a turnover. On the other hand, only 48 percent of the restatements prompted by the SEC or other parties result in a turnover. The difference between the turnover rates for company- and auditor-prompted restatements relative to restatements prompted by other parties is statistically significant (p-value = 0.02). On the other hand, the stock market and accounting measures of firm performance (either in the year before or the year after the restatement) are not significantly different across the company- and auditor-prompted restatements relative to other restatements. For example, mean *ROA* for company- and auditor-prompted restatement firms is −6.3 percent in the fiscal year prior to the restatement. The corresponding *ROA* for other restatement firms is −3.6 percent (p-value for the difference = 0.65). Similarly, there is no significant difference in the stock market performance of the company- and auditor-prompted restatement firms relative to the rest of the sample, either in the year before or the year after the restatement. Taken together, these findings suggest that the impact of a restatement on management turnover is incremental to the impact of firm performance on turnover.

In Panel B of Table 5, we show that the our results hold even if we re-estimate the multivariate turnover model similar to that presented in Panel B of Table 4 that includes the performance and governance controls. Specifically, we estimate a multivariate turnover model using the specification reported in Panel B of Table 4 with the addition of a binary

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[17] We also examine the magnitude of the restatements and find that the total amount (i.e., the sum of annual and quarterly adjustments) is largest for company- and auditor-prompted restatements. The median company-prompted restatement is 22 cents per share and the median auditor-prompted restatement is 20 cents per share. The median per-share amount for all other prompters is 17 cents.

TABLE 5
Auditor- and Company-Prompted Restatement Analysis

Panel A: Performance and Turnover by Prompter

Measure	Auditor- and Company-Prompted Restatements	All Other Restatements	Test Statistic	p-value
ROA	−0.063	−0.036	0.45	0.65
3-day Return	−0.124	−0.087	1.04	0.30
Management Turnover	57 of 84 (68%)	30 of 62 (48%)	5.62	0.02

Panel B: Logistic Regression—Dependent Variable is Management Turnover (0,1)

Variable	Coeff	p-value	Coeff	p-value
Intercept	−0.90	<0.01	−0.02	0.97
RESTATE	−0.43	0.21	−0.69	0.19
*RESTATE*AFTER*	1.80	<0.01	0.94	0.03
*CONTROL*AFTER*	0.73	<0.01	0.07	0.86
CHM/CEO	−0.78	<0.01	−0.99	<0.01
AUDCOMPPROMPTER	0.71	<0.01	0.78	0.04
STKPERF1	—	—	−0.35	0.08
STKPERF2	—	—	−0.40	0.05
ROA	—	—	−1.95	<0.01
BLKOWN	—	—	<0.01	0.57
INSOWN	—	—	<−0.01	0.62
Pseudo-R^2	0.117		0.156	
Number of Observations	584		324	

Panel A reports performance and management turnover for the sample firms, partitioned by the party prompting the restatement. Management turnover in Panel A is measured over the 24 months following the announcement of the restatement. Management turnover is defined as the change in any individual holding the title of Chairman, CEO, and/or President. ROA is measured as of the year preceding the restatement announcement and 3-Day Return is the three-day share price response to the restatement announcement.

Management turnover in Panel B is measured over the period of 24 months prior to the announcement of a restatement in the pre-restatement period and 24 months following the announcement of a restatement in the post-restatement period.

AUDCOMPPROMPTER = 1 for firms having restatements prompted by the company or the auditor, and 0 for all other restatements (and for control firms).

The remaining Panel B variables are the same as in Table 4.

variable (*AUDCOMPPROMPTER*) that takes the value of 1 for firms having company- or auditor-prompted restatements, and 0 for other restatement firms as well as the control firms. In this specification *AUDCOMPPROMPTER* is positive and significant (p-value = 0.04), confirming that company- and auditor-prompted restatements are associated with higher levels of turnover. However, note that *RESTATE*AFTER* retains its significance as well (p-value = 0.03), showing that management turnover in the aftermath of an earnings restatement is significantly higher even after controlling for firm performance, ownership structure, and the party prompting the restatement.

Thus, overall, our results strongly suggest that restatements significantly increase the likelihood of management turnover and that the impact of restatement is incremental to the impact of performance on turnover. Even so, it is likely that an important motivation for firms to engage in earnings manipulation may be to mask poor performance, with those getting caught being those that could not hide the poor performance any longer. Under these circumstances, it may be difficult to fully disentangle the two effects. We acknowledge this limitation of our study.

Subsequent Employment for Displaced Managers

Our analysis of management turnover suggests that the managers of restatement firms are disciplined by the board, as evidenced by increased turnover following the restatement. However, for such discipline to be effective, it is necessary that the managerial labor market also views the departure as informative and imposes further discipline in the form of *ex post* settling up. The models of adverse selection in the labor market argue that firms will try to prevent a turnover of high-ability workers; therefore, the fact that a turnover has occurred may imply that the displaced individual may have lower ability (Greenwald 1986). This suggests that in addition to facing discipline from internal sources (resulting in turnover), managers also face discipline from external sources such as the managerial labor market (in form of *ex post* settling up). Fama (1980) argues that such an *ex post* settling up in the labor market has the potential to discipline managers and influence managerial behavior, *ex ante*.

Prior research that has examined top management turnover following bankruptcy and cash tender offers provides evidence consistent with *ex post* settling up in the labor market for top managers (Gilson 1989; Agrawal and Walkling 1994). A recent article in the *Economist* also seems to suggest that the labor market imposes significant costs on the displaced managers, as evidenced by the following excerpt:

> A sacked CEO, says Tom Neff, chairman of Spencer Stuart and doyen of America's recruiters of chief executives, "may be literally unemployable." He is extremely unlikely ever to run another public company, although he may be able to "hang on to a board or two" as a non-executive, or to gain a seat on the board of a couple of unimportant companies. Hardly anyone returns from the dead. (The *Economist* 2003, 13)

In order to test whether *ex post* settling up exists for the managers of restatement firms, we tracked each of the displaced managers of the sample and control firms using various sources. In essence, we are interested in determining what career paths these managers took following their displacement. Were they able to obtain comparable jobs at public companies? Were they able to obtain any similar jobs at all (in public or private companies)? Was the future career path different for managers of sample firms relative to control firms?

We used multiple sources to track each of the 112 displaced sample firm managers and 63 displaced control firm managers. First, we performed a Google search for each individual, followed by a Factiva search of all major U.S. newspapers and periodicals. We then searched for each person in four databases that provide information on executives and directors. The four databases that we used were Hoover Online, Board Analysts, Forbes.com's Peopletracker, and Standard & Poor's Net Advantage. While there was a significant overlap in these data sources, we nonetheless attempted to verify the new employment using the various filings of the new employer.

The results of our subsequent employment analysis are presented in the Post-Period Turnover columns of Table 6, Panel A. We use three measures of subsequent employment. In the first measure (*REHIRE1*), we examine whether the displaced managers obtained subsequent employment at another public firm as either President, CEO, or Chairman. In the second measure, we expand the definition to include employment at either a private

TABLE 6
Rehire Analysis

Panel A: Subsequent Employment for Sample and Control Firm Managers

	Pre-Period Turnover		χ^2	Post-Period Turnover		χ^2
	Sample	**Control**	**p-value**	**Sample**	**Control**	**p-value**
Total Managers Displaced	40	38	—	112	63	—
Comparable Employment at Public Firm (*REHIRE1*)	11 (27.5%)	12 (31.6%)	0.69	17 (14.9%)	13 (20.6%)	0.36
Hired at Public/Private Firm (*REHIRE2*)	19 (47.5%)	14 (36.7%)	0.34	32 (28.6%)	31 (49.2%)	<0.01
Hired at Public/Private Firm/ Directorships (*REHIRE3*)	22 (55.0%)	20 (52.6%)	0.83	37 (33.0%)	38 (60.3%)	<0.01
Not rehired or unable to trace the individual	18 (45%)	18 (47.4%)		75 (67%)	25 (39.7%)	

Panel B: Logistic Regression for Turnover Firms

	Dep. Variable is *REHIRE1*		Dep. Variable is *REHIRE2*		Dep. Variable is *REHIRE3*	
Variable	**Coeff**	**p-value**	**Coeff**	**p-value**	**Coeff**	**p-value**
Intercept	0.95	0.53	2.22	0.11	2.10	0.12
RESTATE	0.42	0.59	1.00	0.19	0.36	0.66
*RESTATE*AFTER*	−1.62	0.02	−1.73	<0.01	−2.06	<0.01
*CONTROL*AFTER*	−1.02	0.11	0.16	0.79	−0.44	0.47
STKPERF1	−0.37	0.21	−0.09	0.51	−0.10	0.48
STKPERF2	−0.03	0.91	0.39	0.10	0.47	0.05
AGE	−0.02	0.39	−0.04	0.11	−0.02	0.48
FIRMSIZE	0.16	0.24	0.23	0.22	0.20	0.15
*AGE*FIRMSIZE*	<−0.01	0.25	<−0.01	0.20	<−0.01	0.17
TENURE	−0.36	0.16	−0.53	0.02	−0.56	<0.01
Pseudo-R^2	0.077		0.116		0.132	
Number of Observations	190		190		190	

Panel C: Analysis of Wealth Effects for Managers Rehired in Comparable Positions at Public Firms (*REHIRE1*)

	Sample Firms	**Control Firms**
Median (*NEWSIZE/OLDSIZE*)	0.70 (n = 17)	1.90 (n = 13)
Median (*NEWSALARY/OLDSALARY*)	0.95 (n = 10)	0.67 (n = 6)
Percent of observations where *NEWSIZE* < *OLDSIZE*	64.71% (n = 17)	46.15% (n = 13)
Percent of observations where *NEWSALARY* < *OLDSALARY*	64.71% (n = 17)	83.33% (n = 6)

(*continued on next page*)

TABLE 6 (Continued)

Panel A reports management turnover for sample and control firms over the period of 24 months before and after the restatement announcement. Management turnover is defined as the change in any individual holding the title of Chairman, CEO, and/or President within 24 months of the restatement. Control firms are selected based on industry, age, and size.

Panel B variable definitions are as follows:

AGE = age of displaced manager at the time of the restatement;
FIRMSIZE = market value of equity one month prior to restatement; and
TENURE = 0, 1, or 2 if the number of years as officer prior to turnover is in the bottom quartile, between the bottom and top quartile, or in the top quartile of the distribution, respectively.

RESTATE, CONTROL, STKPERF1, and *STKPERF2* are as defined in Table 4.

Panel C compares size and salary for the new employer relative to the old employer (the restating firm or control firm). We use total assets to measure size, and we adjust both the size and salary measures for inflation. The number of observations in the salary comparisons is less due to missing salary information or zero salaries (e.g., for non-executive Chairmen).

firm or at any capacity at a public firm (*REHIRE2*). *REHIRE2* is a cumulative measure as it includes all managers who find subsequent employment at a public firm as either the president, CEO, or Chairman (*REHIRE1*) as well as those individuals who find employment at a private firm or at any capacity at a public firm. Our third measure (*REHIRE3*) includes individuals who are employed as nonemployee directors at public firms. Similar to *REHIRE2*, *REHIRE3* is also a cumulative measure.

We find that only 17 out of 112 (15 percent) managers of the sample firms were rehired at comparable positions afterward. This rehire rate is similar to the 12.5 percent rehire rate reported by Fee and Hadlock (2004) for S&P 500 executives who departed their firms around a scandal and were able to obtain a job with an executive rank at another public firm. The corresponding rate for the managers of the control firms in our sample is 21 percent (13 out of 63), but the difference relative to sample firms is not significant. When we look at a broader definition of rehire (*REHIRE2*), however, we find that an additional 15 sample firm managers were employed either at private firms or at public companies in various capacities (except the top three positions). For the control firms, the corresponding number is 18. Thus, for *REHIRE2*, we find that 32 (17 + 15) of the 112 displaced managers of the sample firms (29 percent) are able to find some form of employment at private or public firms following their departure. The corresponding number for the control firms is 31 (13 + 18) of 63 (49 percent), which is significantly different from the rate for sample firms (p-value = 0.01).

Finally, when we expand the definition of rehire further (*REHIRE3*) we find that an additional five of the displaced sample firm managers and seven control firm managers show up only as nonemployee directors of companies. Thus, the total number of sample firm managers coded as *REHIRE3* is 37 (17 + 15 + 5) and for the control firms is 38 (13 + 18 + 7), such that the *REHIRE3* rate for the sample firms is 33 percent (37 out of 112) and the corresponding rate for the control firms is 60 percent (38 out of 63). This difference is significant at the 1 percent level. It is also important to note that the Pre-Period Turnover columns of Table 6, Panel A show no significant pre-restatement differences between sample and control firms in any of the three rehire measures. Overall, then, the results from Panel A of Table 6 show (1) that the future employment prospects for the sample firm managers are significantly worse than those of the control firm managers and (2) that the sample firm managers' prospects only worsened materially after their firms disclosed accounting restatements. In addition to these reputational penalties, four of the sample firm managers are either under investigation or indicted or were sentenced to prison.

It is likely that variables such as firm performance, manager age, and tenure on the job affect a displaced manager's ability to secure comparable employment at another firm. Thus, in Panel B of Table 6 we present the association between rehiring and restatement after controlling for these and other factors. Furthermore, because it is possible that some unobserved firm-specific differences may affect the rehire rates across the sample and control firms, we use a difference-in-differences model that is identical in spirit to the model presented in Table 4. We estimate separate multivariate models for each of the three definitions of *REHIRE*.

The results of the univariate tests presented in Panel A of Table 6 are further confirmed by our multivariate analysis. Panel B of Table 6 shows that the rehire rate of sample firm managers declines significantly following the restatement after controlling for various other factors that may affect the likelihood of subsequent employment. Specifically, the results show that the coefficient on *RESTATE*AFTER* is negative and significant for each of the three definitions of *REHIRE*, indicating that a restatement adversely affects the future employment prospects of displaced sample firm managers. In addition to the restatement, tenure of the manager at his/her last job appears to negatively affect the likelihood of subsequent employment, consistent with findings in Fee and Hadlock (2004). One interpretation of the negative association between tenure and the rehire rate is that a firm's willingness to part with a manager who has substantial firm-specific capital is viewed as a negative signal by potential future employers. Another possibility is that managers who have longer tenure are presumed to have been more directly responsible for the conditions giving rise to the restatement, in which case it is more likely that they would be viewed as "damaged goods" in the labor market. Overall, the results point to a significant reputational penalty imposed by the managerial labor market on the managers of restating firms.

An Analysis of Wealth Effects for Displaced Managers

In the analysis presented above, our measure of the labor market penalty for the managers of restating firms was in terms of the reduced likelihood of subsequent employment. To further buttress our argument that a restatement results in an *ex post* settling up in the managerial labor market, we report some statistics on the quality of employment for those managers who were able to secure subsequent comparable employment in Panel C of Table 6. The results in Fee and Hadlock (2004) suggest that the quality of subsequent employment for displaced managers as measured by the relative size of the former and future employers as well as the relative salary at the former and future employers is poor, and that the quality of subsequent employment is particularly poor for involuntary departures. To provide some evidence on the quality of subsequent employment for displaced managers in our study, we undertake a similar analysis. Since we require salary data to conduct this analysis, we restrict this analysis to those managers who were rehired as either the president, CEO, or Chairman at another public firm (*REHIRE1*). However, our discussion of the quality of subsequent employment is brief as not only is our sample size much smaller than Fee and Hadlock (2004), but also the firms in our sample are rather small (Fee and Hadlock's sample consists of firms in the S&P 500 Index) which makes it very difficult to obtain detailed data on management compensation.

Following Fee and Hadlock (2004), our first proxy for the quality of subsequent employment is the ratio of firm size (total assets) of the new employer relative to that of the old employer for *REHIRE1* managers. Given that compensation is significantly related to firm size (Murphy 1999), this measure is a reasonable proxy for the quality of new employment. The statistics reported in Panel C of Table 6 show that the for the 17 managers of the sample firms who were subsequently rehired at one of three top positions, the median ratio of new employer firm size to old employer firm size is 0.70, while the corresponding ratio for the 13 control firm managers is 1.90. This suggests that for the managers of sample firms who are rehired, the relative quality of the new employment appears to be poor.

Our second proxy for the quality of subsequent employment is the ratio of the salary at the new employer to the salary at the previous employer. Given that salary data are not available for all managers, the sample size for this analysis is smaller. For the sample firms, the salary ratio is computed for only 10 out of 17 managers. Two of the managers served only as Chairman and did not draw a salary at the restatement firm and five managers receive a salary of *zero* at their new employer (one of them is a non-executive Chairman). Similarly, we exclude three control firm managers for whom we were unable to find compensation data in their new jobs and three more are excluded as they served as non-executive Chairman in their previous job. The median value of this ratio for the 10 sample managers with available data is 0.95, compared to 0.67 for the seven control firm managers. Although the ratio is higher for sample firm managers, the fact that five of the rehired managers receive a salary of zero is indicative of the quality of the new employment. Moreover, we find that the median total cash compensation for the sample firm managers drops from $187,341 in their previous job to $141,346 (not adjusted for inflation) in their new job. For the control firm managers, their median total cash compensation increases from $234,557 in their previous job to $588,394 in their new job. The results of this analysis also suggest that the quality of the new employment for sample firm managers is poor, both relative to their previous jobs and relative to the quality of new jobs for the control firm managers.

Overall, the results of the above analysis, though noisy, suggest that the restatement firm managers who do manage to find new jobs tend to experience deterioration in job quality. Furthermore, our analysis likely understates the true extent of the quality loss, given that we are unable to gather data for managers who are rehired at lower-ranking positions in public firms or at smaller private firms. These results in general provide corroborative evidence of *ex post* settling up or reputational penalties in the labor market for displaced managers.

Post-Restatement Changes in Governance

The results in the previous sections show that, on average, boards of restating firms act in a decisive manner following the revelation of earnings restatement by removing the top managers at a majority of the firms. However, it is likely that in addition to removing top management, the board and the company also take other actions to improve the governance of the company and thereby try to restore the confidence of investors and other stakeholders. A recent paper by Farber (2005) examines changes in various governance mechanisms following the issuance of AAERs by the SEC for firms that violated SEC's Rule 10b-5 over the period 1982–2000. He documents significant changes in various measures of corporate governance following the issuance of AAERs, such as an increase in the proportion of outside directors, reduced incidence of the same individual holding titles of Chairman and CEO, increased number of audit committee meetings, etc.

In this section we briefly examine whether the restating firms instituted changes in their governance structures following their restatements. We examine changes in four key variables: the incidence of the same individual holding the title of Chairman and CEO, the proportion of outside directors, ownership of officers and directors, and ownership of 5 percent and greater blockholders. Recall that prior to the restatement, as shown in Table 2, the governance structures of the sample and control firms had only one important difference—the proportion of outside directors was significantly lower for restatement firms.

Table 7 reports changes in the governance variables following the restatement using the third proxy statement (or second, if the third is not available) issued after the restatement. The proportion of outside directors at the sample firms increases from 61 percent to 69 percent following the restatement, and increases from 71 percent to 76 percent among the control firms. Both of these changes are statistically significant. Thus, although, the proportion of outside directors does increase at the sample firms, it remains lower than the corresponding proportion for control firms. The results also show a significant increase in 5 percent blockholders among both sample and control firms in the post-restatement period. The mean ownership of 5 percent blockholders at the sample firms increases from 18 percent to 22 percent, while the corresponding increase at the control firms is from 22 percent to 25 percent.

Overall, the results show that the sample firms experience an increase in two important governance variables following the restatement: the proportion of outside directors and the ownership of 5 percent and greater blockholders. These results are consistent with sample companies trying to improve their governance structures after their restatements. As such, the findings are similar in spirit to the changes in governance structure documented in Farber (2005) for a sample of firms that were targeted by the SEC. However, these findings should be interpreted with caution as there is significant attrition in the sample firms that may bias the results upward.

V. CONCLUSION

In this paper, we examine management turnover following earnings restatements. Our study is motivated by the lack of evidence in prior literature about adverse consequences to the managers of firms that have restated earnings (GAAP violations) or committed other types of corporate fraud. This has prompted various observers to conclude that internal control mechanisms at U.S. firms are incapable of disciplining managers. Concerns have also been expressed regarding the SEC's ability to investigate all of the alleged violators due to resource limitations. Given the above, our study is important because a high managerial turnover rate coupled with a low rehire rate can potentially influence managerial actions and incentives, *ex ante*.

In contrast to prior research, our examination of management turnover following earnings restatements reveals significant penalties for managers of firms that have restated their earnings. Roughly 60 percent of the firms experience a turnover in at least one of three top positions (Chairman, CEO, or President) within 24 months of the announcement of the restatement. The corresponding proportion at the control firms is approximately 35 percent. This increased turnover obtains even after controlling for firm performance and other determinants of management turnover. Moreover, the displaced managers face further discipline from the external labor market as the rehire rate for restatement firm managers is approximately half that of control firm managers. In addition, the quality of the new employment of the sample firm managers appears to be quite poor relative to their previous jobs, as well as relative to the quality of new employment obtained by the control firm managers.

We find our results comforting in that, contrary to popular perceptions, the managers of firms committing GAAP violations do not seem to be getting away with earnings manipulation (with the caveat that our analysis is limited to those cases where the manipulation is revealed). To the contrary, managers of restating firms incur significant losses in power, prestige, reputation, and, presumably, financial wealth for violating or aggressively interpreting GAAP. At a time when investor confidence is at its lowest level in years due to

TABLE 7

Governance Analysis for Restatement and Control Firm Pairs

	Restatement Firms			Control Firms		p-value
Variable	**n**	**Mean**	**Within Sample p-value**	**Mean**	**Within Sample p-value**	**Restatement vs. Control Difference**
% Insider Ownership before	113	26.6	—	24.5	—	0.50
% Insider Ownership after	88	21.6	—	20.8	—	0.80
Chg in Insider Ownership	81	−3.9	0.12	−4.9	<0.01	0.70
5% and Greater Block-holdings before	112	18.0	—	22.1	—	0.12
5% and Greater Block-holdings after	90	22.0	—	25.3	—	0.31
Chg in 5% and Greater Block-holdings	83	4.5	0.05	4.8	0.02	0.92
% Outsiders on the Board before	112	60.7	—	71.1	—	<0.01
% Outsiders on the Board after	89	69.1	—	76.1	—	<0.01
Chg in % Outsiders on the Board	83	8.0	<0.01	4.7	<0.01	0.17
% Chm/CEO before	140	60.7	—	62.1	—	0.81
% Chm/CEO after	79	62.0	—	70.9	—	0.24

As in Table 2, ownership and board structure data are taken from proxy statements. The variables are as defined in Table 2. The data before the restatement are from proxy statements issued prior to the restatement and data for the post restatement period are from the third (or second if the third is not available) proxy issued after the restatement. We require both the sample firm and the control firm to have data for each line item, such that only complete pairs are included in the analysis.

accounting irregularities and the SEC's inability to pursue every violator, our findings are encouraging. Specifically, they suggest that private penalties may serve as partial substitutes for public enforcement of GAAP violations, thereby potentially reducing the costs of enforcement.

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THE ACCOUNTING REVIEW
Vol. 81, No. 1
2006
pp. 113–133

Are Investors Influenced by Pro Forma Emphasis and Reconciliations in Earnings Announcements?

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ABSTRACT: This study presents the results of an experiment that examines how two underlying characteristics of pro forma earnings announcements, pro forma emphasis and the presence of a quantitative reconciliation, influence nonprofessional investors' and analysts' reliance on pro forma disclosures. The results indicate that the emphasis management places on pro forma earnings, not the mere presence of pro forma earnings, influences nonprofessional investors' judgments and decisions, but that this influence is mitigated by the presence of a quantitative reconciliation. Further analysis reveals that the influence of pro forma emphasis on nonprofessional investors' judgments and decisions seems to be the result of an unintentional cognitive effect as opposed to the perceived informativeness of the earnings figure emphasized by management. Analysts' judgments and decisions were also affected by the presence of reconciliation, but in the opposite direction to those of nonprofessional investors. Specifically, the presence of a quantitative reconciliation led analysts to view pro forma earnings as more reliable, increasing their reliance on the pro forma disclosure in judging the earnings performance of the firm.

Keywords: *analysts; nonprofessional investors; pro forma earnings; reconciliations.*

Data Availability: *Contact the author.*

I. INTRODUCTION

This study investigates how characteristics of pro forma earnings announcements influence nonprofessional investors' and analysts' judgments. The specific characteristics I examine are managers' emphasis of non-GAAP earnings measures (i.e., pro forma earnings) and the presence of a quantitative reconciliation of the differences between the GAAP and non-GAAP earnings measures.

This paper is based on my doctoral dissertation completed at the University of Washington. I thank my committee members, Dave Burgstahler, Frank Hodge, John Miyamoto, Ed Rice, and especially my chair, Jane Kennedy. I thank the following for their helpful comments: Bob Bowen, Ted Christensen, Kevin Jackson, Kathryn Kadous, Susan Krische, Dawn Matsumoto, Molly Mercer, Jeff Miller, Mark Peecher, Brad Schafer, Lisa Sedor, Jim Seida, D. Shores, Steve Smith, Arnie Wright, two anonymous reviewers, and workshop participants at Boston College, Emory University, University of Illinois, University of Notre Dame, University of Washington, and the 2004 ABO Research Conference. I gratefully recognize the financial support of the Deloitte & Touche Foundation. Finally, I thank the financial analysts and M.B.A. students who participated in this study.

Editor's note: This paper was accepted by Marlys Lipe, Editor.

Submitted February 2004
Accepted June 2005

Until recently, quarterly earnings press releases provided one of the least constrained venues for public disclosure of accounting data. Managers claim to use this discretion to provide investors with an earnings metric that is more relevant in predicting future performance than GAAP earnings (Henry 2001; Weil 2001b; Nyberg 2004). In contrast, accounting regulators and other critics claim that managers use this discretion to manipulate investors' perceptions of the firm by strategically emphasizing the earnings metric that portrays better (not actual) firm performance and obfuscating the differences between GAAP and pro forma earnings metrics (Pitt 2001; Weil 2001a; SEC 2001). In 2001, prominent industry groups (i.e., National Investors Relations Institute [NIRI] and Financial Executives Institute [FEI]) responded to these claims by issuing guidelines on the use of pro forma reporting. The guidelines suggested that firms provide a reconciliation of the pro forma and GAAP measure, and to prominently display in the headline of the earnings release the most meaningful information—"most often this is the GAAP and/or pro forma earnings per share" (NIRI 2001). In 2002, the NIRI issued additional guidelines that recommended that firms emphasize GAAP earnings over pro forma earnings (NIRI 2002).

The Securities and Exchange Commission (SEC) has been adamant that pro forma earnings should always be reconciled to GAAP earnings in earnings releases, and has stated that presenting pro forma earnings without a reconciliation would "make the presentation of the material non-GAAP measure misleading" (Teach and Reason 2002; SEC 2003; Taub 2003). The SEC endorsed the initial guidelines issued by the NIRI and FEI in 2001 and urged investors to look for explanations of how pro forma results differ from financial statements prepared under GAAP rules, and to make sure that they understand these differences before investing on the basis of pro forma results (SEC 2001). However, they have been reluctant to regulate the positioning of non-GAAP earnings measures in quarterly earnings announcements. In 2003, the SEC adopted Regulation G (SEC 2003), which established rules for the use of non-GAAP earnings metrics. These rules require any non-GAAP information disclosed in an earnings release to be reconciled quantitatively with the relevant GAAP information. If a non-GAAP metric is included in a *filing to the SEC*, then the GAAP information must be presented with the same prominence as the non-GAAP information. Despite these guidelines and rules, companies continue to exercise discretion over these characteristics of pro forma disclosures in non-SEC filings such as earnings announcements. For example, Intuit (NASDAQ: INTU) headlined their third-quarter May 2004 earnings release with a 14 percent growth in pro forma earnings. GAAP earnings per share followed halfway down the page, and the difference between GAAP and pro forma earnings was explained in general terms in a footnote to the release, with details provided in a separate document (Nyberg 2004).[1]

This study presents the results of an experiment that provides evidence on how two underlying characteristics of pro forma earnings announcements, pro forma emphasis and the presence of a quantitative reconciliation, influence nonprofessional investors' and analysts' reliance on pro forma disclosures. Prior research has provided evidence that when pro forma earnings are presented before GAAP earnings, nonprofessional investors' judgments are influenced by pro forma disclosures, while those of analysts are unaffected (Frederickson and Miller 2004). I extend this research in two ways. First, I distinguish whether it is the *presence* or the *emphasis* of pro forma disclosures that influences nonprofessional investors' judgments. Second, I examine if the presence of a quantitative reconciliation of the differences between the GAAP and pro forma earnings measures changes analysts' or nonprofessional investors' reliance on pro forma disclosures.

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[1] Several other S&P 500 companies that continue to give pro forma prominence in earnings releases are BMC Software (NYSE: BMC), Chiron (NASDAQ: CHIR), Hewlett-Packard (NYSE: HPQ), PeopleSoft (NASDAQ: PSFT), Sabre Holdings (NYSE: TSG), and Sanmina (NASDAQ: SANM) (Nyberg 2004).

In my experiment, M.B.A. students (hereafter nonprofessional investors) and sell-side analysts (hereafter analysts) judged the earnings performance of a hypothetical firm in the technology sector based on an earnings press release and made a subsequent investment decision. The earnings announcement contained either GAAP disclosures alone or both GAAP and (higher) pro forma disclosures. When both types of disclosures were provided, either the GAAP or pro forma metric was emphasized. Finally, when the pro forma metric was emphasized, a quantitative reconciliation was either present or absent in the earnings announcement. The GAAP disclosure was sufficiently detailed to allow participants to calculate the pro forma earnings figure reported in each of the other conditions. Thus, differences in participants' judgments should not be attributable to differences in information availability across conditions.

Consistent with expectations, nonprofessional investors' judgments and decisions were influenced by the emphasis of pro forma earnings, not the mere presence of the pro forma disclosure. Specifically, when pro forma earnings were disclosed, nonprofessional investors assessed earnings performance to be higher and invested a larger amount when the firm's earnings release emphasized pro forma earnings relative to GAAP earnings than when the firm's earnings release emphasized GAAP earnings relative to pro forma earnings. Additionally, the emphasis management places on pro forma (GAAP) earnings does not appear to influence nonprofessional investors' perceptions of the relevance of the two earnings metrics. This latter finding suggests that the influence of pro forma emphasis is the result of an unintentional cognitive effect as opposed to the perceived informativeness of the earnings figure emphasized.

While it appears managers can influence nonprofessional investors' perceptions of their earnings reports by strategically emphasizing pro forma earnings in earnings announcements, this influence is mitigated by the presence of a quantitative reconciliation. Specifically, when the firm's earnings release emphasized pro forma earnings and reconciled those earnings with lower GAAP earnings, nonprofessional investors assessed earnings performance to be lower and invested a smaller amount than when no reconciliation was provided. In contrast, analysts assessed earnings performance to be higher and invested a larger amount when the firm's earnings release emphasized pro forma earnings and reconciled those earnings with GAAP earnings. Additional analysis indicates that the presence of a reconciliation affected analysts' earnings performance judgments through its effect on their perceptions of the reliability of pro forma earnings.

This study extends prior academic research in two ways. I extend research on strategic disclosure by examining the effects of emphasizing information in quarterly earnings press releases. Although prior research (e.g., Schrand and Wong 2000; Frederickson and Miller 2004) provides evidence that investor judgments are influenced by the type of information provided in quarterly earnings releases, these studies do not focus on the emphasis of information provided. In addition, I add to prior research on analysts, and more broadly investor sophistication, by providing evidence that investors' perceptions of the relevance and reliability of voluntary disclosures likely depend on their level of sophistication. Specifically, for analysts, I provide evidence that the transparency of the disclosure influences their perceptions of information reliability.

This study contributes to practice by providing evidence consistent with concerns expressed by prominent industry groups that the judgments of less-sophisticated investors may be influenced by the emphasis of pro forma disclosures. This study also provides evidence to regulators that the influence of pro forma emphasis on less-sophisticated investors' judgments may be mitigated by the prominent placement of a quantitative reconciliation of GAAP and pro forma earnings. Finally, this study provides evidence to managers that more-sophisticated investors, such as analysts, are more likely to rely on pro forma disclosures when a quantitative reconciliation of the two earnings metrics is included in the earnings announcement.

The remainder of the paper is structured as follows. Section II discusses prior relevant research. Section III presents the hypotheses development. Section IV describes the experiment. Section V discusses the results and additional analyses. Section VI presents conclusions, limitations, and suggestions for future research.

II. BACKGROUND

In response to the debate over pro forma reporting, a number of studies have investigated the market's response to these alternative earnings metrics. Bhattacharya et al. (2003) examine a sample of pro forma earnings releases and find that pro forma earnings are more informative and persistent than GAAP earnings, suggesting that market participants view pro forma earnings as more representative of "core earnings" than GAAP earnings. Lougee and Marquardt (2004) find similar results when GAAP earnings informativeness is low or when strategic considerations are absent (e.g., GAAP earnings surprises are positive). Johnson and Schwartz (2003) compare a sample of firms that report pro forma earnings to a sample that only report GAAP earnings and provide evidence that the former firms trade at a market premium. However, they find that the magnitude of the premium is unrelated to characteristics of the pro forma disclosure itself.[2]

These studies provide evidence of an association between pro forma earnings and stock prices, but do not address prominent industry groups' and regulators' concerns that less sophisticated investors' judgments are influenced by the pro forma disclosure itself. Frederickson and Miller (2004) extend these archival findings by investigating whether the equity valuation judgments of two classes of investors, analysts and nonprofessional investors, are affected by pro forma disclosures. The authors find that nonprofessional investors' stock price judgments are affected by pro forma disclosures when pro forma earnings are presented first in an earnings press release, while those of analysts are unaffected.[3] However, at least two empirical questions remain: (1) Is it the *presence* or *emphasis* of pro forma disclosures that influences nonprofessional investors' judgments? and (2) Does the *presence* of a quantitative reconciliation change investors' (i.e., analysts or nonprofessionals) reliance on pro forma disclosures?

III. HYPOTHESES DEVELOPMENT

Emphasis of Pro Forma Earnings Metrics

When reporting pro forma earnings, managers can choose to simply mention pro forma earnings or to emphasize pro forma earnings relative to GAAP earnings in the earnings announcement. Prior research has shown that nonprofessional investors are influenced by pro forma disclosures, while analysts' are unaffected (Frederickson and Miller 2004). However, prior research does not distinguish if it is the mere presence or the emphasis of pro forma disclosures that influences nonprofessional investors' judgments. It is likely that nonprofessional investors will respond to the emphasis of the pro forma metric, while analysts will be unaffected by the emphasis (or presence) of the pro forma metric.

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[2] Bhattacharya et al. (2003) investigate why their findings—that pro forma earnings are incrementally informative to GAAP earnings—differ from the results reported in Johnson and Schwartz (2003) and suggest that the differences in the findings can be attributed to sample size and statistical power.

[3] Frederickson and Miller (2004) investigate the case where the reported pro forma profit is greater than the reported GAAP profit and is presented first in the press release. They do not manipulate the relative emphasis of the two earnings metrics or the presence (absence) of reconciliation.

Prior judgment and decision-making research has shown that the emphasis of information (i.e., its salience or prominence), regardless of its informativeness or usefulness, can have unintentional effects on information processing (Taylor and Fiske 1978; Taylor and Thompson 1982; Fiske and Taylor 1991; Hirshleifer et al. 2004). Salient information attracts users' attention and thus can affect the weight users assign to that information in making a judgment or decision (Dreben et al. 1979; Duncan 1980). In an attempt to reduce cognitive effort, an individual might use or overweight a less important cue simply because it is emphasized (Wallsten 1977, 1980; Taylor and Fiske 1979; Fiske and Taylor 1991). Managers can exploit this salience effect to influence investors' perceptions of the firm by emphasizing the earnings metric that portrays better firm performance.

Nonprofessional investors and analysts are likely to respond differently to the emphasis of pro forma earnings because these two investor groups possess different levels of investment-related expertise and thus approach investment tasks differently (Maines and McDaniel 2000). Prior research has shown that unintentional cognitive effects, such as overweighting salient information, are more likely when individuals lack task-specific knowledge (Joyce and Biddle 1981; Smith and Kida 1991). Most nonprofessional investors have ill-defined valuation models and possess little knowledge about the importance of specific financial statement items or the relations among financial statement items (SRI International 1987; Maines and McDaniel 2000). In addition, it is unlikely that nonprofessional investors possess a complete understanding of pro forma earnings and their relation to GAAP earnings, or have experience in using pro forma earnings to value a firm (Henry 2001; Pitt 2001; Weil 2001b; Bryan-Low 2002; Taub 2003; Teach and Reason 2002). Hence, nonprofessional investors may *unintentionally* rely on the pro forma metric when it is emphasized simply because it is more salient, even if they do not fully understand the metric or perceive the information to be relevant to the judgment task.[4] To summarize, I expect that the emphasis management places on pro forma earnings will influence nonprofessional investors' judgments.[5]

H1: When pro forma earnings are disclosed, the emphasis of pro forma earnings relative to GAAP earnings will influence nonprofessional investors' judgments.

Prior research also finds that task-specific knowledge or expertise often mitigates or eliminates unintentional cognitive effects such as overweighting salient information (Joyce and Biddle 1981; Smith and Kida 1991). In contrast to nonprofessional investors, analysts typically have well-defined valuation models and possess task-specific knowledge about the importance of and relations among financial statement items (Hunton and McEwen 1997; Maines and McDaniel 2000; Jacoby et al. 2001; Schipper 2001). In addition, analysts claim to understand the differences between GAAP and pro forma earnings and often incorporate both earnings measures into their valuation and forecasting models for the firms that they follow (Alpert 2001; Henry 2001; Weil 2001a). Thus, analysts are unlikely to *unintentionally* or *intentionally* rely on the pro forma metric simply because it is emphasized in the earnings announcement. In fact, prior research finds that analysts' stock price judgments are unaffected by pro forma disclosures even when pro forma earnings are presented before GAAP earnings in a press release (Frederickson and Miller 2004). To summarize, I do not expect analysts' judgments will be influenced by the emphasis or presence of pro forma earnings. As a replication of prior research, I examine whether this expectation holds true.

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[4] An alternate prediction is that nonprofessional investors may *intentionally* rely on the pro forma metric because they believe its emphasis is a signal from management that it is more informative than the GAAP metric. However, prior research suggests that nonprofessional investors' stock price judgments are influenced by pro forma disclosures through unintentional cognitive effects, rather than a conscious decision to rely on the pro forma disclosure because of its perceived informativeness (Frederickson and Miller 2004). Although the purpose of this paper is not to determine if nonprofessional investors *unintentionally* or *intentionally* rely on pro forma disclosures, I provide supplemental evidence to help disentangle these two possibilities.

[5] It is unclear whether the presence of the pro forma metric will have an effect on nonprofessional investors' judgments independent of the emphasis effect. To determine whether the mere presence, in addition to the emphasis of pro forma disclosures, influences nonprofessional investors' judgments, I test separately for each of these effects.

Reconciliation of GAAP and Pro Forma Earnings Metrics

Given that nonprofessional investors and analysts are likely to respond differently to the emphasis of pro forma metrics, it is likely that these two investor groups will also be differentially influenced by the presence of a quantitative reconciliation of GAAP and pro forma earnings. Specifically, the presence of a simultaneous reconciliation of GAAP and pro forma earnings will likely reduce nonprofessional investors' reliance on the pro forma disclosure, while analysts' reliance will likely be unaffected.

I state above that the emphasis management places on pro forma earnings will likely influence nonprofessional investors' judgments. Focusing on salient information is an automatic process; however, the process of evaluating and integrating subsequent information is cognitively effortful (Gilbert et al. 1988; Chapman and Johnson 2002). One way to mitigate the bias from overweighting salient information is to "repackage" the information so that the evaluation and integration processes consume less cognitive resources (Dietrich et al. 2001). Making information available is not sufficient; the same available information can be displayed in ways that will make the information easily processable and increase or decrease its impact on judgments and decisions (Magat et al. 1986; Schkade and Kleinmuntz 1994; Payne et al. 1993). An example of "repackaging" available information to make it easily processable is displaying related information simultaneously (Russo 1977). Simultaneous presentation of related information directs users' attention toward examining relations among the information items (e.g., the relation between GAAP and pro forma earnings) and reduces the cognitive effort of integrating the related information (Russo 1977; Hodge et al. 2004). Consequently, users will likely evaluate more thoroughly related information that might otherwise be ignored or discounted due to its dispersed placement within a larger body of information (Hirshleifer et al. 2004; Hodge et al. 2004).

When a simultaneous reconciliation of GAAP and pro forma earnings is present, the information may be structured so that the two earnings measures are presented simultaneously in adjacent columns and are explicitly reconciled. Thus, the cognitive costs related to evaluation and integration are reduced as the relation between GAAP and pro forma earnings is apparent. Specifically, the types of charges excluded from GAAP earnings to arrive at pro forma earnings and their amounts are more apparent to investors. In order to readily see the same relation when a reconciliation is absent, the decision maker must possess knowledge of the relation between GAAP and pro forma earnings and expend the cognitive effort necessary to rearrange and transform the earnings information.

While analysts are likely familiar with the task of transforming financial information to ease evaluation and integration tasks, in general, as it is a core activity of their profession

(Bouwman 1982; Bouwman et al. 1987; Hunton and McEwen 1997), nonprofessional investors do not have the same amount of experience and/or training. In fact, prior research has found that for these types of tasks, cognitive-processing difficulties are more prevalent in nonprofessional investors. For example, in contrast to analysts who simultaneously integrate relevant information, nonprofessional investors often fail to integrate related financial information from different parts of the information display (Bouwman 1982; Bouwman et al. 1987). Prior research has shown that certain technologies such as XBRL (eXtensible Business Reporting Language) can mitigate these cognitive processing difficulties by bringing together pieces of related information (Hodge et al. 2004). A simultaneous reconciliation of GAAP and pro forma earnings serves a similar function by increasing the salience of the relation between the two earnings metrics. Like the XBRL technology, the reconciliation, in part, substitutes for the knowledge that nonprofessional investors lack or cognitive resources nonprofessional investors are unwilling to expend by explicitly displaying the differences between the two earnings metrics and highlighting relations they may not automatically consider. Thus, the presence of a simultaneous reconciliation is likely to lead nonprofessional investors to reduce their reliance on the salient pro forma metric by drawing attention to the relation between GAAP and pro forma earnings and decreasing the cognitive resources necessary to transform and integrate this related information. This leads to the following hypothesis for nonprofessional investors:

> **H2:** When pro forma earnings are disclosed, the presence of a quantitative simultaneous reconciliation will reduce the influence of the emphasis of pro forma earnings on nonprofessional investors' judgments.

In contrast to nonprofessional investors, most analysts possess an understanding of pro forma earnings and their relation to GAAP earnings (Alpert 2000; Henry 2001; Weil 2001a). In addition, analysts are familiar with the task of transforming financial information to ease evaluation and integration tasks and often engage in the task of simultaneously integrating relevant information (Bouwman 1982; Bouwman et al. 1987; Hunton and McEwen 1997). Thus, analysts' judgments are unlikely to be affected by a simultaneous reconciliation if its only purpose is to substitute for this knowledge and task-specific expertise that analysts possess.[6] This leads to the following hypothesis for analysts:

> **H3:** When pro forma earnings are disclosed, the presence of a quantitative simultaneous reconciliation will have no effect on analysts' judgments.

IV. EXPERIMENTAL METHOD

Participants

Eighty-nine first-year M.B.A.s from a large state university participated in the experiment as nonprofessional investors.[7] Fifty-five sell-side analysts with an average of 8.6 years experience in technology-related industries were recruited from Nelson's Directory of Investment Research (2001) to participate in the experiment as analysts. As expected, analysts had more experience than M.B.A.s in analyzing financial statements (10.23 versus 1.76 years; $t = 6.76$, $p < 0.01$), investing in mutual funds (9.23 versus 2.99 years; $t = 4.64$, $p < 0.01$) and investing in individual securities (12.03 versus 2.03 years; $t = 7.38$, $p < 0.01$), for firms in general. Also as expected, analysts had more familiarity with pro forma earnings, in general, than M.B.A.s. Specifically, more analysts than M.B.A.s reported having heard of the term "pro forma earnings" (100 versus 80 percent; $\chi^2 = 11.52$, $p < 0.01$), understanding pro forma earnings was an earnings metric sometimes reported in addition to GAAP earnings (100 versus 68 percent; $\chi^2 = 20.04$, $p < 0.01$), knowing the items typically excluded from GAAP earnings to arrive at pro forma earnings (96 versus 21 percent; $\chi^2 = 70.40$, $p < 0.01$), and analyzing the financial reports (94 versus 18 percent; $\chi^2 = 72.30$, $p < 0.01$) and making an investment decision (86 versus 15 percent; $\chi^2 = 80.00$, $p < 0.01$) for firms that report pro forma earnings. Together, these findings suggest that analysts are, in general, more experienced investors than M.B.A.s and are, on average, more familiar with and more expert in analyzing the financial reports of a firm that reports pro forma earnings to make an investment decision.

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[6] Hirst and Hopkins (1998) provide evidence that analysts' valuation judgments can be influenced by a fundamental variation in the way accounting information is reported in the financial statements. Specifically, Hirst and Hopkins (1998) examine the influence of clear display of income components and their placement on analysts' detection of earnings management. In contrast, the purpose of the reconciliation in this study is to highlight the relation between GAAP and pro forma earnings. Thus, display of income components and their financial statement placement are not varied in this study.

[7] Previous research uses M.B.A.s as proxies for nonprofessional investors (e.g., Hirst et al. 1999; Maines and McDaniel 2000; Hodge 2001; Frederickson and Miller 2004; Hodge et al. 2004).

Press Release

All participants viewed an earnings press release for a hypothetical firm in the technology sector named X Technologies. The earnings press release was modeled after actual reports from companies in the technology sector that trade on the NASDAQ and report both GAAP and pro forma earnings. The press release announced earnings for the first quarter ended February 28, 2002 and began with a headline stating "X Technologies Reports 1st Quarter Results," followed by a subheading announcing either reported GAAP or pro forma Earnings Per Share (EPS) for the first quarter, depending on the experimental condition.[8] Next, reported sales were announced followed by a brief description of X Technologies and a narrative that stated comparative quarterly earnings for the first quarter ended 2002 and 2001.

For the first quarter ended 2002 (2001), X Technologies reported a GAAP loss of \$1.02 (\$1.29) per share and a pro forma profit (loss) of \$0.01 (\$0.12) per share.[9] The press release narrative also contained a list of the items excluded from GAAP earnings to arrive at pro forma earnings in every condition where pro forma earnings were reported.[10] Finally, comparative financial statements for the first quarters ended February 28, 2002 and 2001 were presented.

The items excluded from GAAP earnings to arrive at pro forma earnings were chosen based on earnings announcements of actual firms in the technology sector and are some of the most common items excluded as reported in previous archival research (Bhattacharya et al. 2003, 2004; Bowen et al. 2005). Items excluded are listed below:

- Stock-based Compensation Charges
- Amortization of Purchased Intangibles

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[8] I purposely chose the term "pro forma" earnings and not terms such as "operating earnings" or "ongoing earnings," which participants may have easily confused with "operating income," which has a clear definition under GAAP. Bhattacharya et al. (2003) find that "pro forma" is the most commonly used term and captures approximately half of all non-GAAP earnings figures.

[9] Bhattacharya et al. (2004) find that, for a sample of pro forma announcements from 1998 to 2000, 13.5 percent turned a GAAP loss into a pro forma profit.

[10] Dollar amounts for each exclusion were not given in the narrative. However, each exclusion was a line item on the GAAP Statement of Operations and thus all participants had the information available to calculate reported pro forma earnings. Thus differences in judgments should not be attributable to differences in information availability across conditions.

- Acquired In-Process Research and Development
- Amortization of Goodwill and Other Intangibles
- Restructuring Charges
- Gain on Sale of Investments
- Equity Investment Losses

Experiment Design and Task

I used a 2 × 4 between-subjects design with one cell eliminated. I manipulated the presence of the pro forma disclosure, the emphasis of the pro forma disclosure, and the presence of a quantitative simultaneous reconciliation as described below. In addition, I used two investor types: nonprofessional investor (i.e., M.B.A. student) and analyst.

Each condition contained quarterly GAAP earnings information and a balance sheet. The presence and emphasis of the quarterly pro forma earnings disclosure, and the presence or absence of a quantitative simultaneous reconciliation varied depending on the experimental condition. In the GAAP ONLY condition, quarterly GAAP earnings information was presented in the headline, followed by a discussion of the quarterly GAAP earnings information in the text, and a GAAP income statement in the body of the earnings press release. In the GAAP EMPHASIS (PRO FORMA EMPHASIS, NO RECONCILIATION) condition, quarterly GAAP (PF) earnings information was presented in the headline, a discussion of quarterly GAAP (PF) earnings preceded a discussion of quarterly pro forma earnings (GAAP) information in the text, and a GAAP (PF) income statement preceded a pro forma (GAAP) income statement in the body of the earnings press release.

In the PRO FORMA EMPHASIS, RECONCILIATION condition, similar to the PRO FORMA EMPHASIS, NO RECONCILIATION condition, quarterly pro forma earnings information was presented in the headline, and a discussion of quarterly pro forma earnings information preceded a discussion of quarterly GAAP earnings information in the text of the earnings press release. However, in the body of the press release, the quantitative quarterly GAAP and pro forma earnings information was displayed simultaneously in a reconciled format as opposed to sequentially with no reconciliation. The quantitative reconciliation was a side-by-side presentation of the quantitative GAAP and pro forma earnings information in a single table where the differences between the two metrics were explicitly reconciled. Exhibit 1 presents the reconciliation shown to these participants.

Procedure

M.B.A.s were randomly assigned to one of the four conditions described above and received a single piece of paper containing instructions and a web address for the case on one side and a preview of the first question set on the reverse side. I did not expect analysts' judgments to be influenced by the emphasis of a particular earnings metric; therefore, analysts were randomly assigned to only three of the four conditions (GAAP ONLY; PRO FORMA EMPHASIS, NO RECONCILIATION; or PRO FORMA EMPHASIS, RECONCILIATION) and electronically received a link to the web address for the study. After typing in the address, all participants viewed the same instructions, a general introduction, and a preview of the first question set. Next, participants clicked on a "Begin" button that presented them with the press release for X Technologies and a link to "Question Set I" where they were provided earnings performance judgments and investment amount decisions. After completing Question Set I, participants submitted their responses electronically by clicking on a button labeled "Submit." Participants then clicked on links to "Question Set II" and "Question Set III" and submitted their responses in the same manner. Question Set II elicited participants' perceptions of the relevance and reliability of the reported

EXHIBIT 1
Reconciliation Provided in PRO FORMA EMPHASIS, RECONCILIATION Condition

X TECHNOLOGIES
PRO FORMA CONSOLIDATED STATEMENT OF OPERATIONS
(in millions, except per share data)
(unaudited)

	Three months ended					
	February 28, 2002			February 28, 2001		
	As Reported (1)	Pro Forma Adjustments	Pro Forma	As Reported (1)	Pro Forma Adjustments	Pro Forma
Net sales	$361.80	$—	$361.80	$426.10	$—	$426.10
Cost of sales	230.10	—	230.10	436.40	—	436.40
Gross profit (loss)	**131.70**		**131.70**	**(10.30)**		**(10.30)**
Operating expenses:						
Research and development	48.60	—	48.60	58.90	—	58.90
Selling, general and administrative	77.30	—	77.30	98.20	—	98.20
Stock-based compensation	94.50	(94.50)	—	95.60	(95.60)	—
Amortization of purchases intangibles	693.80	(693.80)	—	741.30	(741.30)	—
Acquired in-process research and development	132.40	(132.40)	—	128.10	(128.10)	—
Amortization of good will and other intangibles	337.20	(337.20)	—	377.60	(377.60)	—
Restructuring	124.20	(124.20)		142.10	(142.10)	
Total operating expenses	**1,508.00**	**(1,382.10)**	**125.90**	**1,641.80**	**(1,484.70)**	**157.10**
Income (loss) from operations	**(1,376.30)**	**1,382.10**	**5.80**	**(1,652.10)**	**1,484.70**	**(167.40)**
Interest and other income, net	11.20	—	11.20	9.70	—	9.70
Gain on sale of investments	4.50	(4.50)	—	6.40	(6.40)	—
Equity investment losses	(22.50)	22.50	—	(25.80)	25.80	—
Net income (loss)	**($1,383.10)**	**$1,400.10**	**$17.00**	**(1,661.80)**	**1,504.10**	**(157.70)**
Net income (loss) per share	**($1.02)**		**$0.01**	**($1.29)**		**($0.12)**
Shares used in computation of loss per share	1,360.00		1,360.00	1290.00		1290.00

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(1) In accordance with U.S. generally accepted accounting principles (GAAP).

earnings metrics and the transparency of the disclosure. Question Set III contained several manipulation checks and demographic questions. Exhibit 2 shows selected questions from Question Set I and II.

Dependent Measures

I collected data on two earnings performance dependent variables: judgments of current earnings performance and judgments of future earnings potential (see item #1 and #2 in Exhibit 2). A Cronbach coefficient alpha score of 0.74 (0.75) for analysts (M.B.A.s) reveals that the current earnings performance measure and the future earnings potential measure likely measure the same underlying construct. Thus, I report the results of the hypotheses tests for a single combined earnings performance dependent measure, based on the average of the current earnings performance and future earnings potential measure for each participant.[11] I also collected data on an investment decision dependent variable: investment amount (see item #3 in Exhibit 2).

V. RESULTS

Manipulation Checks

Data collected in the post-experimental questionnaire reveal that 90 percent of M.B.A.s and 94 percent of analysts recognized that X Technologies reported GAAP earnings. Only one M.B.A. and one analyst failed to recognize that X Technologies reported pro forma earnings in the conditions where pro forma earnings were reported. Of the participants in the reconciliation condition, 86 percent of M.B.A.s and 94 percent of analysts reported that X Technologies provided a reconciliation of GAAP and pro forma earnings.[12]

Descriptive Statistics and Confirmation of Prior Research

Descriptive statistics for the earnings performance and investment amount dependent measures for each condition by investor type are presented in Panel A of Table 1.[13] Consistent with prior research (Frederickson and Miller 2004), M.B.A.s' earnings performance judgments and investment amounts are higher in the PRO FORMA EMPHASIS, NO RECONCILIATION (PFENR) condition (45.71, \$1050.00) than in the GAAP ONLY (GO) condition (34.04, \$652.17) ($t = 2.52$, $p < 0.01$, one-tailed; $t = 1.75$, $p = 0.04$, one-tailed). This finding confirms that M.B.A.s' judgments are influenced by pro forma disclosures, but does not distinguish if it is the mere presence or the emphasis of pro forma disclosures that influences their judgments. Also consistent with prior research findings (Frederickson and Miller 2004), analysts' earnings performance judgments and investment amounts are no different in the PRO FORMA EMPHASIS, NO RECONCILIATION (PFENR) condition (31.94, \$93.75) than in the GAAP ONLY (GO) condition (28.82, \$187.50) ($t = 0.57$, $p = 0.57$, two-tailed; $t = 0.91$, $p = 0.37$, two-tailed). This finding confirms that analysts' judgments are unaffected by the presence or emphasis of pro forma disclosures, supporting the design decision to omit analyst participants in the GAAP EMPHASIS condition.

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[11] Results of the hypotheses tests for the current earnings performance and future earnings potential measures separately are inferentially identical to the results obtained using the combined earnings performance measure.

[12] Exclusion of the participants who did not pass the manipulation checks yields inferentially identical results.

[13] In results not reported, I run nonparametric tests and find inferentially identical results to those obtained using parametric tests.

EXHIBIT 2
Selected Questions from Question Set I and II

Earnings Performance Judgments

1. Indicate on the scale below your judgment of X Technologies' earnings performance for the first quarter ended February 28, 2002.

2. Indicate on the scale below your judgment of X Technologies' earnings potential over the next two years.

Participants responded by using a scale marked in increments of ten with endpoints of 0 and 100 labeled "very weak/very strong."

Investment Decision

3. Assume you have $5,000 to invest in a stock. Assume the price of X Technologies' stock is $2.00 per share after the first quarter earnings announcement. Indicate on the scale below how much of the $5,000 you would invest in X Technologies.

Participants responded by using a scale marked in increments of $500 with endpoints of $0 and $5,000 labeled "nothing at all/the entire amount."

Perceptions of Relevance and Reliability

4. Indicate on the scale below how relevant you believe *GAAP (pro forma)* earnings are in assessing X Technologies' sustainable earnings potential.

5. Indicate on the scale below how reliable you believe *GAAP (pro forma)* earnings are in assessing X Technologies' unbiased earnings potential.

Participants responded using a scale marked in increments of ten with endpoints of 0 and 100 labeled "not at all relevant (reliable)/very relevant (reliable)."

Perceptions of Informative versus Misleading Nature of Charges Excluded from GAAP

6. X Technologies stated *pro forma* earnings excluding the items listed in the table below. Please indicate whether you believe each item was excluded from *pro forma* earnings in order to provide investors with a measure of sustainable earnings (i.e., recurring earnings) or to influence the investor's opinion or behavior in a favorable direction (i.e., convey biased earnings information).

Participants responded by checking for each item whether its exclusion from pro forma earnings (1) provided information on *sustainable earnings* or (2) was an instance of *strategic reporting.*

Perceptions of Usefulness of Presentation of Earnings Information

7. Indicate on the scale below how useful you believe X Technologies' presentation of earnings information is in understanding and using the information.

Participants responded using a scale marked in increments of ten with endpoints of 0 and 100 labeled "not at all useful/very useful."

Hypotheses Tests

Emphasis of Pro Forma Earnings Metrics

Hypothesis 1 predicts that when pro forma earnings are disclosed, the emphasis of pro forma earnings relative to GAAP earnings will influence nonprofessional investors' judgments. To evaluate H1 and also determine the impact of pro forma earnings presence, I test

TABLE 1
Descriptive Statistics and Analysis of Earnings Performance Judgments and Investment Decisions

Panel A: Descriptive Statistics (mean [standard deviation])

		Earnings Announcement Condition[a]						
Investor Type[b]	**n[c]**	**GAAP ONLY (GO)**	**n**	**GAAP EMPHASIS (GE)**	**n**	**PRO FORMA EMPHASIS, NO RECONCILIATION (PFENR)**	**n**	**PRO FORMA EMPHASIS, RECONCILIATION (PFER)**
M.B.A.								
Earnings Performance	26	34.04 [14.77]	21	38.57 [15.63]	21	45.71 [17.05]	21	34.29 [15.99]
Investment Amount	23	652.17 [552.57]	20	675.00 [712.21]	20	1050.00 [916.23]	19	447.37 [468.29]
Analyst								
Earnings Performance	17	28.82 [16.73]	18			31.94 [15.82]	20	43.00 [17.35]
Investment Amount	16	187.50 [309.57]	16			93.75 [271.95]	18	333.33 [420.08]

Panel B: Planned Comparisons for M.B.A.s (H1 and H2)

	t-statistic	df	p-value[d]
H1 (Test 1: emphasis)			
Earnings Performance: PFENR (45.71) > GE (38.57)	1.41	40	0.08 (1T)
Investment Amount: PFENR (1050.00) > GE (675.00)	1.45	38	0.07 (1T)
(Test 2: presence)			
Earnings Performance: GO (34.04) = GE (38.57)	1.02	45	0.31 (2T)
Investment Amount: GO (652.17) = GE (675.00)	0.12	41	0.91 (2T)
H2			
Earnings Performance: PFER (34.29) < PFENR (45.71)	2.24	40	0.02 (1T)
Investment Amount: PFER (447.37) < PFENR (1050.00)	2.60	37	0.01 (1T)

Panel C: Planned Comparison for Analysts (H3)

	t-statistic	df	p-value[d]
H3			
Earnings Performance: PFER (43.00) = PFENR (31.94)	2.04	36	0.05 (2T)
Investment Amount: PFER (333.33) = PFENR (93.75)	1.95	32	0.06 (2T)

(*continued on next page*)

TABLE 1 (Continued)

[a] In the GAAP ONLY (GO) condition, quarterly GAAP earnings information was presented in the headline, followed by a discussion of the quarterly GAAP earnings information in the text, and a GAAP income statement in the body of the press release. In the GAAP EMPHASIS (GE) [PRO FORMA EMPHASIS, NO RECONCILIATION (PFENR)] condition, quarterly GAAP [PF] earnings information was presented in the headline, a discussion of quarterly GAAP [PF] earnings preceded a discussion of quarterly pro forma earnings [GAAP] information in the text, and a GAAP [PF] income statement preceded a pro forma [GAAP] statement of operations in the body of the press release. In the PRO FORMA EMPHASIS, RECONCILIATION (PFER) condition quarterly pro forma earnings information was presented in the headline, and a discussion of quarterly pro forma earnings information preceded a discussion of quarterly GAAP earnings information in the text of the press release. In the body of the press release, the quantitative quarterly GAAP and pro forma earnings information was displayed simultaneously in a reconciled format as opposed to sequentially with no reconciliation.

[b] Participants provided judgments of earnings performance using a scale marked in increments of ten with endpoints of 0 and 100 labeled "very weak/very strong." Participants provided investment amounts using a scale marked in increments of $500 with endpoints of $0 and $5,000 labeled "nothing at all/the entire amount."

[c] Not all participants provided responses to both the earnings performance and investment amount questions; thus the reported n differs across the two dependent measures.

[d] One-tailed p-values are labeled (1T). Two-tailed p-values are labeled (2T).

separately for the effects of emphasis (test 1) and presence (test 2) of the pro forma earnings metric on nonprofessional investors' judgments. Results for both tests are reported in Panel B of Table 1. Test 1 indicates that M.B.A.s' earnings performance judgments and investment amounts are larger in the PRO FORMA EMPHASIS, NO RECONCILIATION (PFENR) condition (45.71, $1050.00) than in the GAAP EMPHASIS (GE) condition (38.57, $675.00) ($t = 1.41$, $p = 0.08$, one-tailed; $t = 1.45$, $p = 0.07$, one-tailed).[14] Thus, M.B.A.s' judgments and decisions are influenced by the emphasis of pro forma earnings relative to GAAP earnings, supporting H1. Test 2 indicates that M.B.A.s' earnings performance judgments and investment amounts are no different in the GAAP ONLY (GO) condition (34.04, $652.17) than in the GAAP EMPHASIS (GE) condition (38.57, $675.00) ($t = 1.02$, $p = 0.31$, two-tailed; $t = 0.12$, $p = 0.91$, two-tailed), suggesting that M.B.A.s' judgments and decisions are not influenced by the mere presence of pro forma earnings. Together, these results indicate that the emphasis management places on pro forma earnings, not the mere presence of pro forma earnings, influences M.B.A.s' earnings performance judgments and investment decisions.

Reconciliation of GAAP and Pro Forma Earnings Metrics

Hypothesis 2 predicts that when pro forma earnings are disclosed, the presence of a quantitative simultaneous reconciliation will reduce the influence of the emphasis of pro forma earnings on nonprofessional investors' judgments. Results presented in Panel B of Table 1 indicate that M.B.A.s' earnings performance judgments and investment amounts in the PRO FORMA EMPHASIS, RECONCILIATION (PFER) condition (34.29, $447.37) are smaller than in the PRO FORMA EMPHASIS, NO RECONCILIATION (PFENR) condition (45.71, $1050.00) ($t = 2.24$, $p = 0.02$, one-tailed; $t = 2.60$, $p = 0.01$, one-tailed), consistent with H2. Thus, the influence of pro forma emphasis on M.B.A.s' judgments and decisions seems to be mitigated by the presence of a quantitative simultaneous reconciliation.

Hypothesis 3 predicts that when pro forma earnings are disclosed the presence of a quantitative simultaneous reconciliation will have no effect on analysts' judgments. Panel C of Table 1 presents results indicating that analysts' earnings performance judgments and investment amounts in the PRO FORMA EMPHASIS, RECONCILIATION (PFER) condition (43.00, $333.33) are larger than in the PRO FORMA EMPHASIS, NO RECONCILIATION (PFENR) condition (31.94, $93.75) ($t = 2.04$, $p = 0.05$, two-tailed; $t = 1.95$, $p = 0.06$, two-tailed), inconsistent with H3. This finding indicates that analysts' reliance on pro forma disclosures increases when a quantitative simultaneous reconciliation is present in the earnings announcement.

Additional Analyses

Emphasis and Perceptions of the Relevance of GAAP versus Pro Forma Earnings

The results of H1 suggest that the emphasis management places on pro forma earnings, not the mere presence of pro forma earnings, influences M.B.A.s' earnings performance judgments and investment decisions. This influence may reflect an unintentional reliance on the pro forma metric simply because it was salient or an intentional reliance on the pro forma metric because it was perceived to be more informative. To disentangle these two possible explanations, I examine M.B.A.s' perceptions of the relevance of GAAP and pro forma earnings (see item #4 in Exhibit 2).

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[14] While it is possible that participants in the conditions where GAAP earnings were emphasized (GO and GE) and those in the conditions where pro forma earnings were emphasized (PFENR and PFER) are thinking of different specific earnings measures, the investment results suggest that participants assessed the firm's general performance differently as predicted.

M.B.A.s rated GAAP earnings as no more relevant when GAAP earnings were emphasized in the earnings announcement (60.00) than when pro forma earnings were emphasized (66.67) (t = 1.20, p = 0.25, two-tailed). In addition, M.B.A.s rated pro forma earnings as no more relevant when pro forma earnings were emphasized in the earnings announcement (46.67) than when GAAP earnings were emphasized (48.33) (t = 0.25, p = 0.80, two-tailed). Results from this additional analysis support the former explanation. Specifically, the emphasis management places on pro forma (GAAP) earnings does not appear to influence M.B.A.s' perceptions of the relevance of the two earnings metrics.

The Presence of a Reconciliation and Perceptions of the Relevance and Reliability of Pro Forma Earnings

The results of H2 and H3 indicate that both M.B.A.s' and analysts' judgments are influenced by the presence of a simultaneous reconciliation, but in opposite directions. To provide further insight into these findings, I examine if a change in investors' perceptions of the relevance or reliability of pro forma earnings information mediates the effect of the presence of the quantitative simultaneous reconciliation on their earnings judgments (see item #4 and #5 in Exhibit 2).[15] The FASB's conceptual framework states that relevance and reliability are the two primary qualities that make accounting information useful for decision-making purposes (FASB 1980, Concept Statement No. 2). It is possible that investors' perceptions of pro forma usefulness (i.e., relevance and reliability) will change as the presentation of the differences between GAAP and pro forma earnings becomes more transparent.[16]

According to Baron and Kenny (1986), the following conditions must hold to establish mediation: (1) the independent variable (i.e., presence of a reconciliation) must affect the dependent variable (i.e., earnings performance judgment), (2) the independent variable must affect the mediator variable (i.e., pro forma relevance and reliability judgments), and (3) the mediator variable must affect the dependent variable after controlling for the independent variable and the effect of the independent variable on the dependent variable must be less than in condition (1).

Results reported in Table 1 indicated that the presence of a reconciliation influenced M.B.A.s' and analysts' earnings performance judgments, satisfying condition (1) for both participant groups. Results reported in Panel A of Table 2 indicate that condition (2) is not met for M.B.A.s' judgments of either relevance or reliability (t = 0.51, p = 0.61, two-tailed; t = 0.61, p = 0.55, two-tailed). In addition, Panel A of Table 2 indicates that analysts did not rate pro forma *relevance* any differently when a reconciliation was provided (t = 1.40, p = 0.17, two-tailed), failing condition (2).

In contrast, analysts did rate pro forma earnings as more *reliable* when provided with the reconciliation (t = 3.50, $p < 0.01$, two-tailed), satisfying condition (2). To test condition (3), analysts' earnings performance judgments are regressed on their perceptions' of the *reliability* of pro forma earnings and a dummy variable indicating the presence or absence of a reconciliation. As shown in Panel B of Table 2, the coefficient on *PFRELIABILITY* is

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[15] I provided participants with a definition of relevance (reliability) consistent with the FASB's conceptual framework (FASB 1980, Concept Statement No. 2).

[16] Analysts did, in fact, rate the presentation of the earnings information as more useful (i.e., transparent) when a reconciliation was present (72.22) than when a reconciliation was absent (52.78) (t = 2.67, p = 0.01, two-tailed). In contrast, M.B.A.s did not rate the presentation of earnings information as any more useful when a reconciliation was present (53.81) than when a reconciliation was absent (54.44) (t = 0.10, p = 0.92, two-tailed) (see item # 7 in Exhibit 2).

TABLE 2
Descriptive Statistics and Mediation Tests of Pro Forma Relevance and Reliability

Panel A: Descriptive Statistics (mean [standard deviation])

Investor Type^b	n	PRO FORMA EMPHASIS, NO RECONCILIATION (PFENR)	n	PRO FORMA EMPHASIS, RECONCILIATION (PFER)	t-tests		
					t-statistic	df	p-value (2T)
M.B.A.							
Pro Forma Relevance	18	45.67 [13.72]	21	43.81 [19.87]	0.51	37	0.61
Pro Forma Reliability	18	43.33 [19.10]	21	39.52 [19.87]	0.61	37	0.55
Analyst							
Pro Forma Relevance	18	53.33 [20.00]	18	63.33 [22.75]	1.40	34	0.17
Pro Forma Reliability	18	36.67 [17.15]	18	56.67 [17.15]	3.50	34	0.00

Panel B: Regression of Analysts' Earnings Performance Judgments on Reconciliation and Pro Forma Reliability

$$EPJUDGMENT = \alpha + \beta_1 RECONCILIATION + \beta_2 PFRELIABILITY + \varepsilon$$

Variable	Coefficient	t-statistic	p-value (2T)
α	12.08	0.35	0.73
RECONCILIATION	0.86	0.15	0.88
PFRELIABILITY	0.40	2.69	0.01

RECONCILIATION = dummy variable with value equal to 0 if reconciliation is absent and value of 1 if reconciliation is present;

PRO FORMA RELEVANCE = participant response to "Indicate on the scale below how relevant you believe *pro forma* earnings are in assessing X Technologies' sustainable earnings potential (i.e., recurring earnings)," using a scale marked in increments of ten with endpoints of 0 and 100 labeled "not at all relevant/very relevant"; and

PRO FORMA RELIABILITY = participant response to "Indicate on the scale below how reliable you believe *pro forma* earnings are in assessing X Technologies' unbiased earnings potential," using a scale marked in increments of ten with endpoints of 0 and 100 labeled "not at all reliable/very reliable."

significantly positive (t = 2.69, p = 0.01, two-tailed). In addition, the coefficient on *RECONCILIATION* is insignificant when *PFRELIABILITY* is included in the regression (t = 0.15, p = 0.88, two-tailed), satisfying condition (3). Analysts' perceptions of pro forma reliability fully mediate the effect of the reconciliation on their earnings performance judgments.

Why analysts' perceived reliability is affected by the reconciliation can be understood through analysis of their assessments of the informativeness or misleading nature of certain charges excluded from GAAP earnings to arrive at pro forma earnings (see item #6 in Exhibit 2). The analysts viewed some of these charges differently when a reconciliation was present than when a reconciliation was absent. Specifically, analysts rated restructuring charges, gains on the sale of investments, and equity investment losses as more informative (less misleading) when a reconciliation was present than absent (t = 2.08, p = 0.05, two-tailed; t = 1.74, p = 0.09, two-tailed; t = 2.25, p = 0.03, two-tailed, respectively).

VI. CONCLUSIONS, LIMITATIONS, AND FUTURE RESEARCH

This study presents the results of an experiment that provides evidence on how two underlying characteristics of pro forma earnings announcements, pro forma emphasis and the presence of a reconciliation, influence nonprofessional investors' and analysts' reliance on pro forma disclosures. Results indicate that the emphasis management places on pro forma earnings, not the mere presence of pro forma earnings, influences nonprofessional investors' judgments and decisions, but that this influence is mitigated by the presence of a simultaneous reconciliation. Further analyses reveals that the influence of pro forma emphasis on nonprofessional investors' judgments and decisions is the result of an unintentional cognitive effect as opposed to the perceived informativeness of the earnings figure emphasized by management in the earnings release.

Results also suggest that the presence of a reconciliation increases analysts' reliance on pro forma disclosures. A mediation analysis reveals that the presence of a reconciliation affected analysts' earnings performance judgments through its effect on their perceptions of the reliability of pro forma earnings. In fact, analysts rated the nature of certain charges excluded from GAAP earnings to arrive at pro forma earnings as more informative (less misleading) when a reconciliation was present than when a reconciliation was absent. Thus, the presence of a reconciliation led analysts to view pro forma earnings as more reliable, increasing their reliance on the pro forma disclosure in judging the earnings performance of the firm.

This study is subject to several limitations. First, I limited the amount of information participants received to a press release reporting earnings for the first quarter of fiscal 2002 so that participants could complete the task in a reasonable amount of time. Typically when an investor is evaluating the financial performance of a firm, more information is available and, in general, the information environment is more complex. However, reducing the complexity of the information environment allows me to make stronger inferences about the specific factors that influence participants' earnings performance judgments and maximizes the number of analysts willing to participate in the study.

Second, because participants completed this study in an "out-of-lab" setting, I cannot be certain that participants complied with my instructions to refrain from looking back through the case materials when answering the final question set. Although I have no reason to expect that participants did not comply with my instructions, it is possible that uncontrolled, extraneous factors affected my results.

Third, this experiment included first-year M.B.A.s as surrogates for nonprofessional investors. Although first-year M.B.A.s likely exhibit demographic characteristics similar to those of nonprofessional investors, they likely have less investment-related experience and therefore may not accurately reflect the opinions of actual investors.

Other limitations of this study provide potential avenues for future research. First, I purposely chose a firm in the technology sector. Future research could examine whether these results generalize to established industries where GAAP earnings are considered to be more value-relevant. Second, I purposely chose a simultaneous reconciliation to mimic actual environmental characteristics. However, I cannot disentangle the effects of the simultaneous presentation of GAAP and pro forma earnings from the effects of the explicit reconciliation of the two earnings metrics. Future research could examine the separate effects of these two disclosure characteristics on investor judgments and decisions. Third, a change in analysts' perceptions of the reliability of the pro forma metric when a simultaneous reconciliation is present is just one possible explanation for analysts' increased reliance on this metric. The items that I chose to exclude to arrive at pro forma earnings are common exclusions among firms in the technology sector. Future research could examine whether analysts would be less likely to rely on the pro forma earnings metric, even when a simultaneous reconciliation is provided, if the exclusions were less familiar. Future research could also examine whether a change in analysts' perceptions of related variables, such as management credibility, could have contributed to their increased reliance on pro forma earnings.

Despite these limitations, this study provides evidence that firms can influence the earnings judgments and investment decisions of nonprofessional investors by strategically emphasizing pro forma earnings in an earnings announcement. In addition, this study provides evidence that a quantitative simultaneous reconciliation of GAAP and pro forma earnings reduces the ability of firms to influence nonprofessional investors' perceptions of performance in a specified direction, while enhancing analysts' perceptions of the reliability of the reported pro forma metric.

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THE ACCOUNTING REVIEW
Vol. 81, No. 1
2006
pp. 135–157

Financial Reporting Transparency and Earnings Management

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ABSTRACT: Prior research indicates that greater transparency in reporting formats facilitates the detection of earnings management. The current study hypothesizes and demonstrates that greater transparency in comprehensive income reporting also reduces the likelihood that managers will engage in earnings management in the area of increased transparency. In our experiment, 62 financial executives and chief executive officers decide which available-for-sale security to sell from a portfolio. We manipulate the transparency of comprehensive income reporting and the relationship of projected earnings to the consensus forecast in a 2 × 2 between-subjects design. When projected earnings are below (above) the consensus forecast, participants sell securities that increase (decrease) earnings. However, the rarely used, more transparent format for reporting comprehensive income significantly reduces both income-increasing and income-decreasing earnings management. Participants in the less transparent setting indicate that earnings management attempts will not be obvious to readers, will improve stock prices, and have no effect on management's reputation for reporting integrity. Conversely, respondents in the more transparent condition suggest that earnings management will be obvious to readers, harmful to stock prices, and damaging to reporting reputation. Results of this study suggest that more transparent reporting requirements will reduce earnings management in the area of increased transparency or change the focus of earnings management to less visible methods.

Keywords: *financial reporting; transparency; earnings management; comprehensive income; SFAS No. 130.*

The authors thank Rob Bloomfield, Jan Bouwens, Cheryl Dunn, Peter Easton, Eric Hirst, Lisa Koonce, Robert Knechel, Linda McDaniel, Mark Nelson, Nick Seybert, Dan Stone, Eddy Vaassen, workshop participants at the Financial Accounting Standards Board, Florida State University, Harvard University, Tilburg University, The University of Iowa, University of Kentucky, University of Maastricht, University of Tennessee, and participants at the AAA Doctoral Consortium and the FARS Doctoral Consortium for their comments on earlier versions.

Editor's note: This paper was accepted by Jane Kennedy, Editor.

Submitted November 2004
Accepted August 2005

I. INTRODUCTION

Many studies of reporting transparency examine its effects on users' ability to detect earnings management. These studies generally report that more transparent disclosures (those that are easier to process) lead to greater detection of earnings management (e.g., Hirst and Hopkins 1998). Managers also often lobby for and choose less transparent disclosure formats (e.g., inclusion of expenses and liabilities in the notes and market value changes in the statement of stockholders' equity). This suggests that managers believe there is a benefit derived from limiting some users' ability to detect earnings management. Such a belief is consistent with the view of Fields et al. (2001) that rational managers likely would not engage in earnings management in the absence of expected benefits, and such benefits require that at least some users of accounting information be unable or unwilling to disentangle its effects. If easier detection of earnings management reduces its expected value, then greater reporting transparency should reduce the prevalence of earnings management attempts. We test whether managers' earnings management choices and underlying beliefs are consistent with this logic. By doing so, we can more fully understand determinants of earnings management strategies, and facilitate policy makers' evaluations of the costs and benefits of mandating increased transparency.[1]

Based in part on user group requests for clearer reporting of other comprehensive income items, the Financial Accounting Standards Board issued Statement of Financial Accounting Standards No. 130 (SFAS No. 130, FASB 1997), *Reporting Comprehensive Income*. The exposure draft leading to SFAS No. 130 (FASB 1996) required a performance statement approach for the display of comprehensive income. However, in response to preparer input, the FASB decided to also allow a less transparent display in the statement of stockholders' equity. The large majority of companies currently choose the less transparent display.[2] The FASB and the International Accounting Standards Board (IASB) are reconsidering how to display comprehensive income (FASB 2005) with the objective of improving transparency.

Recent research indicates that users are less likely to detect earnings management when relevant information is disclosed in the less transparent statement of stockholders' equity (e.g., Hirst and Hopkins 1998; Hirst et al. 2004). Contemporaneous research also indicates that property casualty insurance firms with a *history* of selective sale of available-for-sale securities, choice of nonspecialist auditors, lower institutional ownership and analyst coverage, and higher bid-ask spreads, were more likely to select the less transparent display in the statement of stockholders' equity in the first year of implementation of SFAS No. 130 (Lee et al. 2005). This suggests that firms with a history of earnings management and weaker monitoring/corporate governance systems tend to choose the less transparent reporting format. What remains unknown is (1) whether the more transparent reporting format would reduce the likelihood that managers will engage in earnings management in the area of increased transparency, (2) if it does, why managers behave in such a manner, and (3) whether the effects are symmetric for income-increasing and income-decreasing earnings management. The answers to these questions provide a basis for predicting the effects of requiring more transparent comprehensive income reporting standards (e.g., FASB 2005), and developing a more complete theory of earnings management and disclosure policy.

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[1] See Arya et al. (2003) for a discussion of potential costs that can result from transparency improvement.

[2] In a survey of 111 randomly selected firms from the Russell 2000 Index, Mazza and Porco (2004) find that 83 percent report comprehensive income in a statement of stockholders' equity, 3 percent use the one-statement approach, and 14 percent use the two-statement approach.

We conduct an experiment where 59 financial executives and three chief executive officers recommend the sale of an available-for-sale security to raise cash for a principle payment. Using a between-subjects design, we vary the direction of management's incentive to manage earnings (consensus analysts' forecasts either below or above management's projection of earnings) and the transparency of the display of comprehensive income (the less transparent statement of stockholders' equity or more transparent single statement of comprehensive income). When projected earnings are below the consensus forecast, participants increase current income by selling an available-for-sale security with an unrealized gain. When projected earnings are above the consensus forecast, participants decrease current income by selling an available-for-sale security with an unrealized loss. Most importantly, the more transparent format (the single statement of comprehensive income) dramatically reduces, but does not eliminate, the prevalence of both types of earnings management. In debriefing, participants indicate they believe that earnings management through selective sale of available-for-sale securities disclosed in the less transparent statement of stockholders' equity would improve stock price and not harm their reputation for reporting integrity. Conversely, they indicate that earnings management disclosed in the more transparent single statement of comprehensive income would harm both stock price and reporting reputation. These results suggest that more transparent reporting requirements will reduce the likelihood of managers engaging in earnings management in areas of increased transparency or change the focus of earnings management attempts to less visible methods.

This study provides direct causal evidence relevant to the effects of the FASB's current proposal on comprehensive income reporting and other efforts to improve reporting transparency. It is also responsive to Healy and Wahlen's (1999) and Libby et al.'s (2002) suggestion that accounting research examine the characteristics of accounting regulation that encourage or discourage earnings management. Further, when combined with the contemporaneous research by Lee et al. (2005), our study demonstrates the power of using multiple methods to examine the same financial reporting issue (Maines 1995; McDaniel and Hand 1996). Our experiment extends the findings of Lee et al. (2005) by allowing for a direct causal inference relevant to the effects of the FASB's current proposal, isolates income-increasing and income-decreasing earnings management, and provides insight into managers' beliefs concerning stock price and reporting reputation effects that underlie their earnings management decisions. Bolstering the external validity of our results, the complementary findings of Lee et al. (2005) suggest that identified effects in the current study would likely not be swamped in the natural environment or offset by factors not included in our experimental setting.

II. BACKGROUND AND HYPOTHESES

Managing Earnings through Selective Sale of Available-for-Sale Securities

Under the provisions of SFAS No. 115 (FASB 1993) and SFAS No. 130 (FASB 1997), available-for-sale securities are reported at fair value on the balance sheet, and unrealized holding gains and losses on those securities are reported as part of other comprehensive income, which is added to net income to arrive at an all-inclusive income measure known as comprehensive income. When the holder sells the securities, they realize as part of earnings the gain or loss that occurred over the entire holding period. In that same period, in order to avoid double counting of gains and losses in comprehensive income reported over the entire holding period, other comprehensive income includes a reclassification adjustment opposite in sign from the gain or loss reported in earnings. Therefore, management can manipulate earnings, but not comprehensive income, by timing the sale of available-for-sale securities.

Several archival studies find that financial institutions engage in earnings management by strategically timing the realization of gains and losses on investment securities (e.g., Moyer 1990; Scholes et al. 1990; Beatty et al. 1995; Collins et al. 1995). In addition, in a sample of earnings management attempts obtained from a survey of experienced auditors, Nelson et al. (2003) find a number of cases where auditors believe their clients selectively timed recognition of realized gains or losses on investments.

Transparency and Comprehensive Income Reporting

One of the perceived advantages of comprehensive income reporting is increasing the ease of extracting information on gains and losses that preparers were otherwise including directly in equity.[3] For example, the Association for Investment Management and Research (AIMR) urged the FASB to implement the concept of comprehensive income, in part, to create more transparency for the reporting of unrealized gains and losses on marketable securities. The AIMR supported the all-inclusive income concept encompassed by comprehensive income and furthermore supported the requirement for comprehensive income to be reported in a performance statement. In SFAS No. 130, the FASB indicated that presentation of comprehensive income in a performance statement would enhance its visibility, thereby increasing investors' use of the information contained therein. Although the FASB agreed to implement comprehensive income reporting requirements, it failed to require inclusion of other comprehensive income items in a performance statement and display of comprehensive income as the bottom line of a performance statement.

SFAS No. 130 permits three alternative reporting formats, two of which present comprehensive income in a statement of performance by using either the one-statement approach or the two-statement approach. In the former, net income becomes a subtotal within a performance statement that displays comprehensive income as its bottom line. In the latter, a company displays the traditional income statement and a separate statement of comprehensive income that begins with net income. Most companies use the third approach, which presents comprehensive income as part of the statement of stockholders' equity, thereby presenting unrealized gains and losses on available-for-sale securities as part of the changes in the balance sheet account accumulated other comprehensive income.

We define more transparent financial disclosures as those from which needed information can be more easily extracted and more effectively used to understand firm economics. In the context of comprehensive income reporting, transparency relates to the relative prominence given to items of other comprehensive income and total comprehensive income (FASB 1996, 1997). Placement of comprehensive income in a performance statement is considered to be more prominent, and thus more transparent, than placement within a statement of stockholders' equity. Furthermore, the voluminous amounts of non-performance-related financial information reported in a statement of stockholders' equity result in items of other comprehensive income being difficult to extract and often misconstrued as non-performance-related.

Hirst and Hopkins (1998) was the first paper to assess whether comprehensive income reporting format affects users' extraction and use of information on gains and losses on available-for-sale securities. Specifically, they examine whether the reporting of unrealized gains and losses on available-for-sale securities in a statement of stockholders' equity, rather than a separate statement of comprehensive income, diminishes analysts' ability to detect earnings management through selective sale. They conclude that, relative to the statement of stockholders' equity approach, the two-statement approach enhances analysts' ability to detect this type of earnings management activity. In addition, the two-statement approach increases analysts' acquisition of information related to comprehensive income and reduces bias in their valuation judgments. The Hirst and Hopkins (1998) results suggest that a higher level of transparency is achieved when comprehensive income and its components are displayed in a statement of comprehensive income rather than a statement of stockholders' equity.

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[3] Prior to SFAS No. 130, preparers were reporting foreign currency translation adjustments, unrealized gains and losses on available-for-sale securities, and minimum pension liability adjustments directly within a balance shown as a separate component of equity.

In a study focusing on fair value accounting for banks, Hirst et al. (2004) investigate whether differences in the way in which commercial banks measure and report comprehensive income cause systematic variation in bank analysts' investment risk assessments and valuation judgments. They find that the valuation judgments of analysts distinguish between banks with different levels of risk only when fair value changes are measured completely (full fair value accounting) and included in a separate comprehensive income statement (as opposed to a statement of stockholders' equity). Thus, although bank analysts are *aware* of the different interest rate risks assumed by high-risk and low-risk banks, clear performance reporting of the financial implications of these risks increases the likelihood that analysts will *use* the information in their investment-related judgments. Similarly, Bloomfield et al. (2005) find that share prices in an experimental market are influenced more by unrealized gains and losses when they are disclosed in the statement of comprehensive income format.

Maines and McDaniel (2000) use an experiment to examine whether alternative presentation formats affect different stages of nonprofessional investors' processing of comprehensive income information. They view more transparent disclosures as those that promote acquisition and proper interpretation or weighting of information. Their results indicate that nonprofessionals acquire and appropriately evaluate the volatility of unrealized gains reported as part of other comprehensive income, regardless of whether managers display such information in a statement of stockholders' equity or in a separate statement of comprehensive income. However, nonprofessional investors place significant weight on their volatility assessments only when those unrealized gains are reported in a statement of comprehensive income.[4] Maines and McDaniel (2000) also suggest that statement placement, income labeling, net income linkage, and other disclosure features can increase reporting transparency. Taken together, these papers suggest that of the three alternative reporting formats for comprehensive income allowed by SFAS No. 130, the statement of stockholder's equity approach is less transparent than either of the performance statement approaches.

Transparency and Earnings Management

As Hirst et al. (2004) note, managers' frequent lobbying efforts for the option to employ less transparent reporting formats provides indirect evidence that they believe that the costs and benefits of earnings management are affected by disclosure transparency. Maines and McDaniel (2000) analyzed the 281 comment letters received by the FASB on their exposure draft, and 90 percent supported disclosure of comprehensive income items in note form or in the statement of stockholders' equity (see also FASB 1997, paras. 60–62). Preparers also lobbied for the option of note disclosure for employee stock option compensation expense and operating lease liabilities as well as the indirect method statement of cash flows. In each case, the FASB's initial decision required the more transparent disclosure, and the less transparent option was later allowed as a result of preparer pressure. Preparers also regularly choose the less transparent option when the standards allow. For instance, the vast majority of companies display comprehensive income in the statement of stockholders' equity and disclose employee stock option compensation expense in the notes, and report cash flow from operating activities using the indirect method.

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[4] Biddle and Choi (2002) support the need to weight comprehensive income elements in valuation decisions. They find that comprehensive income is more relevant for share returns than traditional net income, and that securities gains and losses provide the greatest incremental value-relevance among the other comprehensive income components.

This behavior suggests that managers believe there is a benefit to choosing less transparent disclosures. Such a belief is consistent with arguments by Lundholm (1999) and Hirst et al. (2003) that companies will be punished for earnings management if earnings management is easier to detect, and the view of Fields et al. (2001) that rational managers likely would not engage in earnings management in the absence of expected benefits, and such benefits require that at least some users of accounting information be unable or unwilling to disentangle the effects of the earnings management. It is also consistent with recent theories developed by Bloomfield (2002) and Hirshleifer and Teoh (2003) predicting that markets will react less to information that is difficult to extract or interpret. Both focus directly on a setting where all information is public, but can differ in cost of extraction or correct interpretation.[5]

Bloomfield (2002) develops the "incomplete revelation hypothesis" based on equilibrium outcomes in noisy rational expectations models (Grossman and Stiglitz 1980). In the model, all traders have the same amount of money and level of risk aversion. Also, noise trading keeps prices from fully revealing information and just enough traders collect information to make their trading gains equal to their information collection costs. In equilibrium, facts that are more costly to extract from public information attract fewer traders' interest and are less completely revealed by market prices. Hirshleifer and Teoh (2003) directly focus on "choices between seemingly equivalent presentations of information," including choices of information location (recognition versus disclosure or choice of statement for recognition). In their model, limits to investor attention result in information that is less easily extracted being absorbed by fewer investors. The limited capacity of less attentive investors also causes them to be unable to compensate for the consequences of their limited attention. All investors are risk-averse, so highly attentive investors (those with greater capacity) are unable to fully exploit mispricing. Through a series of examples, they demonstrate how the market will differentially react to equivalent presentations of accounting information that differ in the attentional resources necessary for extraction.

Both theories suggest that easier detection of earnings management resulting from improved transparency should reduce the expected value of earnings management. In the world described by Bloomfield (2002) and Hirshleifer and Teoh (2003), share prices will react more to managed earnings than to unmanaged earnings when extracting unmanaged earnings requires additional effort. If managers are concerned about share price effects, then requiring greater reporting transparency should reduce the value and prevalence of earnings management attempts in the area of improved transparency. This view is consistent with

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[5] As Fields et al. (2001) note, accounting choice matters to managers when it influences the outcomes of contracts, influences parties other than owners, or influences asset prices. We focus on the latter based on findings in Bhojraj and Libby (2005) and Graham et al. (forthcoming) of the primacy of stock price motivations. Any effects based on contracts or parties other than owners could magnify the effects.

Maines and McDaniel's (2000) emphasis that more transparent disclosures are those that promote acquisition and proper interpretation of information.

The most relevant archival research related to this prediction is Lee et al.'s (2005) contemporaneous study of the initial comprehensive income reporting decisions of publicly traded property-liability insurers. Following prior studies, they identify firms as having a *history* of selective sale of available-for-sale securities (they call these firms cherry-pickers) if there is a significant negative correlation between realized available-for-sale security gains and losses and net income before extraordinary items and realized gains and losses. They find that firms classified as having a *history* of selective sale (cherry-picking), choice of nonspecialist auditors, lower institutional ownership and analyst coverage, and higher bid-ask spreads were more likely to select the less transparent display in the statement of stockholders' equity in the first year of implementation of SFAS No. 130.

While the Lee et al. (2005) study indicates that firms identified as earnings managers are more likely to select the less transparent disclosure, the extent to which the more transparent reporting format would reduce managers' propensity to engage in earnings management remains unknown. Furthermore, use of the correlational measure of earnings management does not allow for distinguishing between income-increasing and income-decreasing earnings management, or assessing managers' beliefs that underlie their earnings management choices. Our study focuses on these issues.[6]

Hypotheses

Archival, experimental, and survey studies conclude that firms manage earnings to meet or exceed analysts' forecasts (e.g., Degeorge et al. 1999; Burgstahler and Eames 2003; Brown 2001; Libby and Kinney 2000; Nelson et al. 2002). Depending on the circumstances, managers can have an incentive to manage earnings up or down. For example, the government case against a former HealthSouth CEO contends that "he picked a desired earnings-per-share number each quarter that met or exceeded Wall Street expectations and then instructed his finance executives to inflate the company's financial results accordingly" (Mollenkamp 2003). In other cases, managers have managed earnings downward in some periods (e.g., Freddie Mac).

In our study, participants are required to select one of five available-for-sale securities to sell, any one of which would result in the firm meeting its liquidity needs. Absent other incentives or penalties, it would be optimal for the participants to choose the security that would carry the additional benefit of meeting the analysts' consensus forecast. Thus, an underlying assumption in our study is that participants will recommend the sale of the security that moves projected earnings toward the consensus forecast. As noted above, prior studies of selective sale of available-for-sale securities cannot distinguish between income-increasing and income-decreasing earnings management. Accordingly, we set the direction of managers' earnings management incentives by manipulating whether projected earnings are below or above analysts' consensus forecast, and then test separately whether increasing the transparency of reporting for the financial statement items in question will decrease the prevalence of both income-increasing and income-decreasing earnings management. Based on the literature and theory discussed in the prior section, we anticipate the following:

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[6] Additionally, Lee et al. (2005, 29) note, "Another limitation of our study is that we consider only one highly regulated industry that uniquely has a fairly significant percentage of firms reporting comprehensive income on a performance statement. The increased prevalence of performance reporting may be due to the nature of property-liability insurers' investment portfolios."

H1a: More transparent disclosure of comprehensive income will reduce managers' tendency to recommend selling available-for-sale securities that increase current income when projected earnings are below analysts' consensus forecast.

H1b: More transparent disclosure of comprehensive income will reduce managers' tendency to recommend selling available-for-sale securities that decrease current income when projected earnings are above analysts' consensus forecast.

These hypothesized effects would produce an interaction between reporting format and analyst consensus forecast. Results supporting our hypotheses would suggest that improved transparency will reduce the prevalence of earnings management in the affected areas.[7]

III. RESEARCH METHOD

In our experiment, experienced financial managers select an available-for-sale security for sale to raise cash for a principle payment on debt. The consensus analysts' forecast and comprehensive income report format are varied in a 2 × 2 between-subjects design.

Participants

Our experimental approach is most similar to Cloyd et al. (1996), who study the effects of managers' incentives at public versus private firms on the importance of tax and book conformance; Kennedy et al. (1998), who study the effects of managers' incentives on alternative liability disclosures; and Bhojraj and Libby (2005), who study the effects of capital market incentives and reporting frequency on managers' investment decisions. As Libby et al. (2002) discuss, these studies, as well as the current study, use experienced managers as participants because their goal is to "peer into the minds" of experienced managers to determine what they have learned in the field about the costs and benefits of financial reporting decisions.

The study participants are attendees at six corporate training sessions offered by a professional consulting firm. The 62 participants include 59 financial managers and three CEOs representing 17 public companies. We present demographic data on Table 1.

Task

The task is designed to achieve three goals. First, the portfolio is designed such that sale of available-for-sale securities can be as easily used to manage earnings up or down. Second, the choice context is designed such that the sale is a costless operational decision; that is, it neither requires a trade-off between reporting goals and cash flows nor involves an unethical or illegal decision. Third, the information necessary for detection of selective sales is available in all disclosure formats, but differs in transparency as demonstrated in prior research.

The case materials ask participants to assume that they work for a public company followed by analysts and places them in the situation next described. It is December 15,

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[7] Several arguments have been made in the literature suggesting a plausible finding of no effect of transparency on earnings management. In efficient capital markets, changes in transparency such as those discussed above should have no effect on share price because at least some investors have access to the information. As a consequence, they should have no effect on earnings management behavior. Similarly, if managers believe that there is no cost to earnings management, and there is any chance of a possible benefit, then rational managers would manage earnings equally under all levels of transparency. Only if managers believe that costs and benefits of earnings management vary by levels of transparency would a requirement to use a more transparent comprehensive reporting format be expected to change earnings management behavior. Additionally, research examining the relationship between disclosure quality and earnings management offers mixed results, as one study indicates a positive relationship (Shaw 2003), while another suggests a negative association (Lobo and Zhou 2001).

TABLE 1
Demographic Data

	Number[a]	Percent
Participants employed by public companies	62	100
Professional certifications held by participants:		
Certified public accountant (CPA)	31	50
Certified management accountant (CMA)	7	11
Certified financial analyst (CFA)	7	11
Certified internal auditor (CIA)	4	6
Compensation schemes of employer companies:		
Straight salary	17	27
Salary plus bonus based on an income-related measure	23	37
Salary plus a bonus based on the company's stock price	22	36
	Mean[b]	**(S.D.)**
Average business experience	22.50	(6.44) years
Average accounting/finance experience	20.69	(7.83) years
Personal wealth invested in employer company stock	31.53	(23.62) percent
Average total assets of employer companies	$92,124.56	(220,975.29) million
Average annual sales of employer companies	$10,200.76	(15,622.24) million

[a] Based on Chi-square testing, neither professional certification nor compensation schemes are significantly different ($p > .10$) across treatment conditions.

[b] Based on ANOVA testing, response means are not significantly different ($p > .10$) across treatment conditions.

2002 and the company needs to sell equity securities in order to raise $80 million for a December 20 principle payment on debt. Each participant is provided with information about five equity securities comprising the available-for-sale securities portfolio (see Table 2, Panel A). Each security has a current market value of $80 million. Sale of the five alternative available-for-sale securities A through E will produce a realized $20 million loss, $10 million loss, no gain or loss, $10 million gain, and $20 million gain, respectively. Beginning with income from operations, Table 2, Panel B summarizes the financial statement effects of the sale of each of the five securities.

Participants next learn that (1) the company projects earnings of $1,000 million before considering the effects of the security sales, (2) the information provided ignores income taxes, (3) they are to assume no tax consequences associated with the sale of any security, and (4) for simplicity, relevant balance sheets and cash flow statements are not included in the case materials.[8] The participants then read the analysts' consensus forecast for the company's 2002 earnings and recommend which security they would sell if the situation arose at their company. We also ask participants to indicate which security the senior executives in their company would prefer to sell and which security sale would result in the highest stock price for their company.

As part of the basis for their recommendations, participants also receive five two-page sets of pro forma financial statements, including a performance statement, statement of stockholders' equity, and an available-for-sale securities note reflecting the expected effects of sale of each of the five securities. We provide each set of financial statements on two

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[8] The participants are not given any information indicating differential expected future returns for the five securities.

TABLE 2
Security Sale Recommendation

Panel A: Choice of Available-for-Sale Securities to Sell

	Portfolio of Available-for-Sale Securities[a]				
	Co. A	**Co. B**	**Co. C**	**Co. D**	**Co. E**
Market value	$80	$80	$80	$80	$80
Cost	100	90	80	70	60
Unrealized gain (loss)	$(20)	$(10)	$0	$10	$20

Panel B: Effect of Security Sale Recommendation

	Security Sale Recommendation[a]				
	Co. A	**Co. B**	**Co. C**	**Co. D**	**Co. E**
Income from operations	$1,000	$1,000	$1,000	$1,000	$1,000
Realized gain (loss) from security sales	(20)	(10)	0	10	20
Net earnings	$980	$990	$1,000	$1,010	$1,020
Other comprehensive income:					
Reclassification adjustment for (gain) loss realized in earnings	20	10	0	(10)	(20)
Comprehensive income	$1,000	$1,000	$1,000	$1,000	$1,000
Unrealized gain (loss) remaining in available-for-sale portfolio	$20	$10	$0	$(10)	$(20)

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[a] Dollar values are stated in millions.

separate pages. Following the normal order in annual reports, the first page includes the performance statement and the second page includes the statement of stockholders' equity and the financial statement note. The Appendix displays excerpts from the experimental materials.

Design

We vary whether the consensus forecast is above or below projected earnings and the transparency of the comprehensive income reporting in a 2 × 2 between-subjects design. We hold the projected earnings before the security sale constant at $1,000 million and vary the analysts' consensus forecast at $1,010 million ($10 million above projected earnings) and $990 million ($10 million below projected earnings). Participants with a consensus forecast of $1,010 million have an incentive to engage in current income-increasing earnings management, and could meet or exceed the forecast by selling Security D ($10 million gain) or Security E ($20 million gain), respectively. In the $990 condition, participants have an incentive to engage in current income-decreasing earnings management, and could meet the consensus forecast by selling Security B ($10 million loss) or beat the consensus forecast by selling Security C ($0 gain/loss), Security D ($10 million gain), or Security E ($20 million gain).

We vary the transparency of the comprehensive income reporting at two levels that reflect the most and least transparent options allowable by SFAS No. 130. In the Statement of Comprehensive Income (SCI) condition, the format displays net earnings and other comprehensive income in a single performance statement with comprehensive income as the bottom line. In the Statement of Stockholders' Equity (SSE) condition, the format displays other comprehensive income and total comprehensive income in the statement of stockholders' equity, while the performance statement includes net earnings as its bottom line (see the Appendix for an illustration of both conditions). For all participants, the only item of other comprehensive income is a reclassification adjustment for the realized (gain) loss on the security sale, which is displayed clearly on the face of the financial statement in which comprehensive income is reported (in either the SCI or SSE).

Procedure

The training takes place at conference room locations in six different cities across the U.S. At each session, participants represent more than one company. The subject matter of the training involves compliance with Sections 302 and 404 of the Sarbanes-Oxley Act of 2002. The same trainer holds each of the six sessions and each session lasts four hours. The trainer is unaware of the study hypotheses.[9]

Before each session, the trainer informs the researchers of the number of expected participants. The researchers then provide the trainer with a stack of portfolio folders, each containing two envelopes. The first envelope contains the consent form and experimental materials, and the second envelope includes the manipulation check and debriefing questions. Within the stack of portfolio folders, we place the four treatment conditions in random order.

At the end of each training day, the trainer asks the participants if they would volunteer to participate in a research study that would take about 20 minutes to complete. All participants in each session agree. The trainer passes out the portfolio folders from the top to the bottom of the stack, and asks the participants to remove the materials from the first envelope (marked Envelope #1) and complete the assignment. Prior to responding to the manipulation check and other debriefing items, participants place and seal all materials back into the first envelope and open the second envelope (marked Envelope #2). Hence, they cannot look back at the case materials to answer the manipulation check and other debriefing questions or change their answers to the security sale questions after becoming aware of the nature of the debriefing questions. Once participants complete the manipulation check and debriefing questions, they seal these materials back into the second envelope and place both envelopes into the portfolio folder. The trainer ensures that participants do not communicate with each other during the study, requests that the participants place the materials back into and seal the envelopes, collects the portfolio folders, and mails them back to the researchers.

IV. RESULTS

Manipulation Checks

In the post-experiment debriefing questionnaire, we ask participants to record the analysts' consensus earnings forecast and comprehensive income reporting format provided in the case. All participants correctly indicate their randomly assigned forecast and format conditions, suggesting that they understand the manipulations. In the debriefing questionnaire, we also ask several questions to ascertain participants' understanding of the requirements of SFAS No. 115 and SFAS No. 130. All participants correctly report that unrealized holding gains and losses on available-for-sale securities should be included as part of other comprehensive income. In addition, all participants correctly indicate that upon the sale of an available-for-sale security, the amount of gain or loss reported in earnings reflects the difference between selling price and historical cost. Also, participants are asked the extent to which they agree with the following statement: "The accounting requirements in SFAS No. 115 provide an opportunity to selectively time the sale of available-for-sale securities to achieve desired income statement results" (1 = strongly agree, 9 = strongly disagree). The mean (standard deviation) response to this statement is 1.11 (0.37), suggesting that participants are aware of earnings management opportunities provided by SFAS No. 115. ANOVA testing indicates no significant response differences across treatment conditions ($p > .30$ for all). Based on the responses to the above questions, we deem the experimental manipulations successful.

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[9] One of the researchers observes the first experimental session to ensure that the trainer understands how to administer the materials. One of the researchers also attends the fourth session, again to ensure that the trainer is properly administering the materials.

Hypothesis Tests

The dependent variable for the hypotheses is the net earnings resulting from the sale of the recommended security (Security A = $980 million, Security B = $990 million, Security C = $1,000, Security D = $1,010 million, or Security E = $1,020 million). A choice of Security A or Security B is indicative of a current income-decreasing strategy. Alternatively, a choice of Security D or Security E is indicative of a current income-increasing strategy. Finally, a choice of Security C indicates a neutral strategy. Table 3 reports frequencies of security sale choices made by the participants in the $990 condition (Panel A) and $1,010 condition (Panel B). Table 4 reports means and standard deviations for the net earnings resulting from the security sale recommendations.

We test our hypotheses using an ANOVA model, with the net earnings resulting from the recommended sale as the dependent variable, and reporting format (SSE and SCI) and consensus forecast ($1,010 and $990) as the independent variables. As indicated on Table 5 (Panel A), the main effect of reporting format is not significant ($p = .203$), the main effect of consensus forecast is significant ($p = .000$), and the interaction term is significant ($p = .000$).[10]

As discussed previously, an underlying assumption in our study is that participants will recommend the sale of the security that moves projected earnings toward the consensus forecast. Consistent with that assumption, when projected earnings ($1,000) are below the consensus forecast ($1,010), the mean reported income of $1,009.35 million indicates an income-increasing strategy. Alternatively, when projected earnings are above the consensus forecast ($990), the mean reported income of $994.19 million suggests an income-decreasing strategy. The mean reported income in the $1,010 ($t = 7.66$, one-sided $p = .000$) and $990 ($t = -6.45$, one-sided $p = .001$) consensus forecast conditions were significantly different from $1,000.

Hypothesis 1a (Hypothesis 1b) predicts that more transparent disclosure of comprehensive income will reduce managers' tendency to recommend the sale of available-for-sale securities that increase (decrease) current income when projected earnings are below (above) analysts' consensus forecast. That is, when an analysts' consensus forecast is different from the company's projected earnings, participants' choice of security sale will be influenced by comprehensive income reporting format such that the more transparent format (SCI) will mitigate earnings management attempts as compared to the less transparent

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[10] Dependent variable responses are not significantly different ($p > .10$) across the following background variables: professional certification (CPA, CMA, CFA, CIA, Other), compensation scheme, years of business experience, years of finance/accounting experience, percent of personal wealth invested in employer company, total assets, annual sales, company number (1 through 17), or training session (1 through 6).

TABLE 3
Participants' Security Sale Recommendation Frequencies

	Security Recommended and Net Earnings Resulting from Sale of Recommended Security				
Report Format	**Co. A $980**	**Co. B $990**	**Co. C $1,000**	**Co. D $1,010**	**Co. E $1,020**
Panel A: Participants Receiving Consensus Forecast of $990					
SSE (n = 16)	0	14	2	0	0
SCI (n = 15)	0	4	11	0	0
Total	0	18	13	0	0
Panel B: Participants Receiving Consensus Forecast of $1,010					
SSE (n = 16)	0	0	0	10	6
SCI (n = 15)	0	0	8	7	0
Total	0	0	8	17	6

TABLE 4
Net Earnings Resulting from Sale of Recommended Security
Mean (Standard Deviation)

	CI Reporting Format[a]		
Consensus Forecast[b]	**SSE**	**SCI**	**Main Effect of Forecast**
$990	991.25 (3.42)	997.33 (4.58)	994.19 (5.02)
$1,010	1,013.75 (5.00)	1,004.67 (5.16)	1,009.35 (6.80)
Main Effect of Format	1,002.50 (12.181)	1,001.00 (6.07)	

Security Selected		**Resulting Earnings**
Co. A	=	$980
Co. B	=	$990
Co. C	=	$1,000
Co. D	=	$1,010
Co. E	=	$1,020

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[a] In the SSE condition, comprehensive income is reported within the statement of stockholders' equity. In the SCI condition, comprehensive income is reported in one statement, labeled "statement of earnings and comprehensive income." In the SSE (SCI) condition, the statement of stockholders' equity is presented on a separate page from the statement of earnings (statement of earnings and comprehensive income).

[b] The analysts' consensus forecast for the company's earnings reflect earnings before the effect of the security sale. In one condition, the forecast is $990 million; in the other condition, the forecast is $1,010 million. In both conditions, unmanaged earnings are $1,000 million.

TABLE 5
ANOVA Analysis

Panel A: Tests of Between-Participants Effects

Source	SS	d.f.	MSE	F-Statistic	p-value
Reporting Format (RF)	34.84	1	34.84	1.66	.203
Consensus Forecast (CF)	3445.27	1	3445.27	164.24	.000
RF * CF	890.43	1	890.43	42.45	.000
Error	1216.67	58	20.98		

Panel B: Hypotheses Tests (one-sided t-tests)

		t-statistic	p-value
H1a: SCI × 1,010 ($1,004.67)	< SSE × 1,010 ($1,013.75)	−4.98	.000
H1b: SCI × 990 ($997.33)	> SSE × 990 ($991.25)	4.21	.000

Panel C: Additional Tests (one-sided t-tests)

		t-statistic	p-value
SCI × 1,010 mean ($1,004.67)	> $1,000 (No EM)[a]	3.50	.004
SCI × 990 mean ($997.33)	< $1,000 (No EM)[a]	−2.26	.041
SSE $1,010 − SSE $990	> SCI $1,010 − SCI $990	6.52	.000[b]
SSE $1,010 − SCI $1,010	> SSE $990 − SCI $990	1.29	.101

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[a] A $1,000 value reflects no earnings management (EM) attempt.

[b] This contrast is equivalent to the interaction term in the ANOVA.

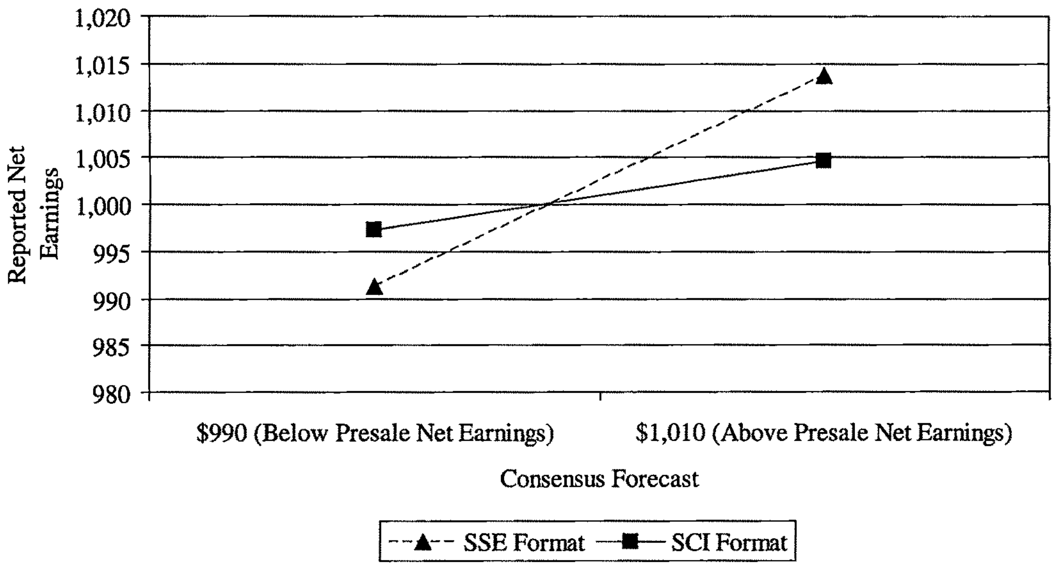
format (SSE). The hypotheses, therefore, predict a significant interaction between the two independent variables. Table 5 (Panel A) indicates a significant (p = .000) interaction of reporting format and consensus forecast. This interaction is equivalent to testing whether the difference in mean reported income between the $1,010 and $990 conditions is greater in the SSE condition than the SCI condition (SSE $1,010 − SSE $990 > SCI $1,010 − SCI $990; t = 6.52, one-sided p = .000). Figure 1 illustrates the nature of the interaction.

We separately test H1a and H1b, for income-increasing and income-decreasing earnings management as shown on Table 5, Panel B. We find that the more transparent reporting format (SCI) significantly dampens managers' tendency to increase current income when projected earnings are below analysts' consensus forecast (H1a: t = −4.98, one-sided p = .000), and decrease current income when projected earnings are above the forecast (H1b: t = 4.21, one-sided p = .000).

We also test whether any income-increasing and income-decreasing earnings management remains in the more transparent SCI conditions (see Table 5, Panel C) by testing whether the mean reported income resulting from the $1,010 SCI condition ($1,004.67) is significantly greater than $1,000, and the mean reported income resulting from the $990 SCI condition ($997.33) is significantly less than $1,000. Both of these tests are significant (t = 3.50, one-sided p = .004; t = −2.26, one-sided p = .041; respectively), suggesting that while the more transparent SCI condition does indeed dampen the tendency toward income-increasing and income-decreasing earnings management, some degree of earnings management persists nevertheless.

Finally, we test whether the difference in reported income between the reporting formats in the $1,010 condition where income-increasing earnings management was in evidence is

FIGURE 1
Mean Net Earnings Resulting from Sale of Recommended Security



greater than the change in the $990 condition where income-decreasing earnings management was in evidence (SSE $1,010 − SCI $1,010 > SSE $990 − SCI $990), and observe a result that is not significant at conventional levels (t = 1.29, one-sided p = .101). This analysis does not suggest a significant asymmetry in the effects of transparency on income-increasing and income-decreasing earnings management. In summary, the experimental results support H1a and H1b, and indicate that the likelihood of earnings management attempts decreases as transparency increases.

Debriefing Analyses

As noted earlier, we also ask participants to indicate (1) which security senior executives in their company would prefer to sell, and (2) which security would result in the highest stock price for their company. There are no significant differences in the security sale recommended by the participants and the security they thought senior executives would sell, and the security they thought would result in the highest stock price. Furthermore, in responses to debriefing questions, all participants indicate that it is very important to them to maintain or improve stock price and reputation for reporting integrity. These responses suggest that participants believe that increases in transparency of reporting decrease the benefits to their company and its shareholders resulting from selective sales of available-for-sale securities.

To further investigate the participants' beliefs that underlie their choices, we ask them to assume that a company used the same comprehensive income format as in the case study (either SCI or SSE), and answer three questions:

1. How obvious would it be to statement readers that a company was using techniques such as realized gains/losses from available-for-sale securities to manage earnings? (1 = Not very; 9 = Very)

2. If a company used techniques such as realized gains/losses from available-for-sale securities to manage earnings, what might be the effect on the company's stock price? (1 = Strong negative; 9 = Strong positive)
3. If a company used techniques such as realized gains/losses from available-for-sale securities to manage earnings, what would be the effect on its reputation for reporting integrity? (1 = Strong negative; 9 = Strong positive)

Table 6 reports participants' mean responses.

Participants indicate that earnings management through selective sale of available-for-sale securities would be much more obvious in the more transparent SCI than the less transparent SSE format conditions (p = .001). Further, earnings management is expected

TABLE 6
Debriefing Analysis

Panel A: How Obvious Would It Be to Statement Readers that a Company was Using Techniques Such as Realized Gains/Losses from Available-for-Sale Securities to Manage Earnings?

	CI Reporting Format	
Consensus Forecast	**SSE**	**SCI**
$990	2.44	8.33
$1,010	3.56	8.40

(1 = Not very; 9 = Very)

Tests of Between-Participants Effects

Source	**SS**	**d.f.**	**MSE**	**F-Statistic**	**p-value**
Reporting Format (RF)	445.95	1	445.95	788.37	.001
Consensus Forecast (CF)	5.50	1	5.50	7.92	.003
RF * CF	4.34	1	4.34	7.66	.008
Error	32.81	58	0.57		

Panel B: If a Company Used Techniques Such as Realized Gains/Losses from Available-for-Sale Securities to Manage Earnings, What Might be the Effect on the Company's Stock Price?

	CI Reporting Format	
Consensus Forecast	**SSE**	**SCI**
$990	5.62	2.00
$1,010	6.56	2.00

(1 = Strong negative; 9 = Strong positive)

Tests of Between-Participants Effects

Source	**SS**	**d.f.**	**MSE**	**F-Statistic**	**p-value**
Reporting Format (RF)	259.50	1	259.50	474.97	.001
Consensus Forecast (CF)	3.40	1	3.40	6.23	.015
RF * CF	3.40	1	3.40	6.23	.015
Error	31.64	58	0.55		

(*continued on next page*)

TABLE 6 (Continued)

Panel C: If a Company Used Techniques Such as Realized Gains/Losses from Available-for-Sale Securities to Manage Earnings, What Would be the Effect on Its Reputation for Reporting Integrity?

	CI Reporting Format	
Consensus Forecast	**SSE[a]**	**SCI[b]**
$990	3.94	1.93
$1,010	4.12	1.73

(1 = Strong negative; 9 = Strong positive)

Tests of Between-Participants Effects

Source	**SS**	**d.f.**	**MSE**	**F-Statistic**	**p-value**
Reporting Format (RF)	74.80	1	74.80	211.07	.001
Consensus Forecast (CF)	0.01	1	0.01	0.01	.967
RF * CF	0.58	1	0.58	1.64	.205
Error	20.55	58	0.35		

[a] Mean (standard deviation) = 4.03 (.047): Mean is not significantly different from the midpoint (4.00) of the scale (t-statistic = 0.72, p-value = .355).

[b] Mean (standard deviation) = 1.83 (.070): Mean is significantly less than the midpoint (4.00) of the scale (t = 4.76, p-value = .001).

to have a positive effect on stock price in the SSE condition and a negative effect in the more transparent SCI condition (p = .001). Earnings management is expected to have no effect on reporting reputation in the SSE conditions (mean response of 4.03 is not significantly different (p = .355) from the midpoint of the scale) and a negative effect in the SCI conditions (mean response of 1.83 is significantly different (p = .001) from the midpoint of the scale. The significant interaction terms related to the first two questions (p = .008 and p = .015, respectively) also indicate greater sensitivity to transparency for income-increasing than income-decreasing earnings management. These responses suggest that managers believe that more transparent reporting will both decrease the benefits and increase the costs of earnings management, which is consistent with theoretical expectations.

V. DISCUSSION

In an experiment involving 62 professional financial managers and CEOs, we examine the extent to which financial reporting transparency affects current income-increasing and income-decreasing earnings management attempts. We find that when financial analysts' consensus forecasts are above (below) management's projected earnings, study participants recommend an income-increasing (-decreasing) earnings management strategy. We also observe that increased reporting transparency significantly dampens but does not eliminate earnings management attempts. These findings are consistent with managers' beliefs that earnings management in less transparent disclosure regimes will improve stock price and not harm reputation for reporting integrity, while earnings management in more transparent disclosure regimes will harm both stock price and reputation for reporting integrity. These results suggest that requiring more transparent financial reporting will reduce earnings management attempts in the areas of increased transparency or shift such attempts to areas of lesser transparency.

Our study contributes to ongoing research associated with earnings management, transparency, and comprehensive income reporting. First, we offer evidence regarding the inclination of managers of nonfinancial public companies to engage in earnings management through the sale of available-for-sale securities—an underlying assumption in the Hirst and Hopkins (1998) study, and a finding for property-liability insurers in Lee et al. (2005). Second, we provide systematic evidence for income-increasing and income-decreasing earnings management in response to the analysts' consensus forecast, whereas most prior studies report the joint effect of both types of earnings management. Third, our study offers the first direct evidence of earnings management attempts from professional managers; in contrast, prior studies (e.g., Libby and Kinney 2000; Nelson et al. 2002) rely on auditors' reports of manager behavior. Fourth, and most important, we demonstrate the effect of financial reporting transparency on earnings management. While Hirst and Hopkins (1998) and others study the impact that alternative methods of display of comprehensive income have on the detection of earnings management, our study investigates whether alternative methods of display have a similar impact on the incentive to manage earnings. Our results, combined with Lee et al.'s (2005) finding of association between comprehensive income reporting transparency and earnings management among property-liability insurers, suggest that a requirement of more transparent disclosure of comprehensive income will reduce earnings management of this type, and possibly move earnings management to less transparent domains.

The results of our study lend support to former SEC Chairman Levitt's (1998) suggestion for improving the transparency of financial statements in an effort to reduce earnings management to meet Wall Street earnings expectations. Given that a majority of companies report comprehensive income in a statement of stockholders' equity (the least transparent format allowed by SFAS No. 130), results from the current study are important for policy makers, such as the IASB and FASB, as they consider changes in the format for reporting comprehensive income. In a major agenda project, the FASB is again addressing the reporting of financial performance and has tentatively decided that companies should be required to report comprehensive income in a single statement of financial performance similar to our single statement of SCI treatment (FASB 2004).[11] Our results suggest these changes would affect the transparency of selective sales of securities and thus the prevalence of this form of earnings management.

We acknowledge certain limitations in our study. First, we neither provide participants with a complete set of financial statements that include the balance sheet and the cash flow statement nor highlight other incentives that may conflict with those related to meeting or beating analysts' forecasts. For example, we instruct participants to ignore tax consequences, even though Cloyd et al. (1996) offer evidence suggesting that managers consider tax consequences as well as financial reporting effects in their decisions. We believe that limiting the financial information in the case materials, asking participants to ignore tax consequences, and designing other simplifications into the experiment are efficient choices from a participant resource perspective, and do not affect the sign of our results.

We offer participants only one earnings management incentive (i.e., the analysts' consensus forecast) and ask them to assume that the only way to raise the needed cash is to sell a security out of the available-for-sale portfolio. In reality, we recognize that managers have various incentives and ways to manage earnings. However, given that prior research suggests that managers do manage earnings by selling from the available-for-sale portfolio, we suggest that requiring participants to raise cash by selling an available-for-sale security is a reasonable setting for examining transparency effects.

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[11] At this time, the FASB has not released an exposure draft or completed due process procedures on this issue.

Our study does not directly test which of the determinants of transparency (e.g., statement placement, income labeling, net income linkage) identified by Maines and McDaniel (2000) have the greatest effect on managers' earnings management decisions. Such tests could be the focus of future research. Additionally, Hodge et al. (2004) suggest that search-facilitating technology such as XBRL (eXtensible Business Reporting Language) improves the transparency of firms' financial statement information. They assert that search-facilitating technology could affect managers' financial reporting choices because it facilitates comparisons across companies in such a way that differing choices become more transparent to users. Such possibilities could also be the focus of future research.

APPENDIX

Excerpts of Materials Provided to Participants

Experimental Task:

Assume that you work for a public company followed by analysts. It is December 15, 2002, and your company needs to sell equity securities in order to raise $80 million to pay principle on debt that is due on December 20, 2002.

Your company owns equity securities in five different companies classified as ***available-for-sale*** under FASB Statement No. 115. As of today (December 15), the securities each have the following cost and market value:

($ in millions)	**Co. A**	**Co. B**	**Co. C**	**Co. D**	**Co. E**
Market Value (total proceeds)	$80	$80	$80	$80	$80
Cost	100	90	80	70	60
Unrealized Gain (Loss)	$(20)	$(10)	$0	$10	$20

You have been asked to make a recommendation about which security should be sold to raise the $80 million cash that is needed to pay debt principle.

When making your recommendation, consider the following additional facts:

(1) Your company projects its earnings before considering the effects of the security sales to be $1,000 million.
(2) For simplicity, the information provided ignores income taxes. In addition, you are to assume that there are no tax consequences associated with your recommendation.
(3) For simplicity, the balance sheets and cash flow statements are not included with the attached materials.

Attached to these instructions are five sets of pro forma financial statements representing the effects of the sale of each alternative security. Please examine and compare each set carefully. Then make your recommendation as you would if this situation arose in your company.

Please answer the following questions considering that:

Analysts' consensus forecast for your company's 2002 earnings is $1,010 [$990] million.

- For my company, I recommend sale of securities of (circle one):
  Co. A Co. B Co. C Co. D Co. E
- Senior executives in my company would prefer sale of securities of (circle one):
  Co. A Co. B Co. C Co. D Co. E
- I believe that the highest stock price for my company would result from sale of securities of (circle one):
  Co. A Co. B Co. C Co. D Co. E

SSE Condition: Example of Financial Statements for Sale of Co. A Security

Page 1: Statement of Earnings
For the Three Months Ended December 31, 2002

Net Sales	$6,000
Less: Cost of goods sold	3,800
Gross profit	2,200
Selling, general and administrative expenses	1,200
Income from operations	1,000
Other gains (losses)	(20)
Net earnings	$980

Page 2: Statement of Stockholders' Equity
For the Three Months Ended December 31, 2002

	Common Stock	Retained Earnings	Accumulated Other Comprehensive Income (Loss)	Total Stockholders' Equity
Balance at September 30, 2002	$14,500	7,200	0	$21,700
Net earnings		980		980
Other comprehensive income:				
Unrealized gains (losses) on available-for-sale securities				
Current period			0	
Reclassification adjustment for (gains) losses recognized in earnings			20	
Net unrealized gain (loss)			20	20
Total comprehensive income				1,000
Balance at December 31, 2002	$14,500	8,180	20	$22,700

Notes to the Financial Statements
December 31, 2002

NOTE 1: INVESTMENT SECURITIES

The fair value and cost of available-for-sale securities as of December 31, 2002 and September 30, 2002 are shown below.

	December 31, 2002	September 30, 2002
Fair value	$320	$400
Cost	300	400
Unrealized gain (loss)	$20	$0

SCI Condition: Example of Financial Statements for Sale of Co. A Security

Page 1: Statement of Earnings and Comprehensive Income
For the Three Months Ended December 31, 2002

Net Sales		$6,000
Less: Cost of goods sold		3,800
Gross profit		2,200
Selling, general and administrative expenses		1,200
Income from operations		1,000
Other gains (losses)		(20)
Net earnings		980
Other comprehensive income:		
Unrealized gains (losses) on available-for-sale securities		
Current period	$0	
Reclassification adjustment for (gains) losses recognized in earnings	20	
Net unrealized gain (loss)		20
Comprehensive income		$1,000

Page 2: Statement of Stockholders' Equity
For the Three Months Ended December 31, 2002

	Common Stock	Retained Earnings	Accumulated Other Comprehensive Income (Loss)	Total Stockholders' Equity
Balance at September 30, 2002	$14,500	7,200	0	$21,700
Net earnings		980		980
Other comprehensive income			20	20
Balance at December 31, 2002	$14,500	8,180	20	$22,700

Notes to the Financial Statements
December 31, 2002

NOTE 1: INVESTMENT SECURITIES

The fair value and cost of available-for-sale securities as of December 31, 2002 and September 30, 2002 are shown below.

	December 31, 2002	September 30, 2002
Fair value	$320	$400
Cost	300	400
Unrealized gain (loss)	$20	$0

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THE ACCOUNTING REVIEW
Vol. 81, No. 1
2006
pp. 159–178

Management of Financial Information in Charitable Organizations: The Case of Joint-Cost Allocations

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ABSTRACT: Charities that use direct mailings or other activities that combine a public education effort with fundraising appeals must allocate the joint costs related to these activities to programs, fundraising, and administration. This study investigates whether charities use joint-cost allocations to manage the program ratio—a widely used measure of spending efficiency. Using a hand-collected dataset of 708 organization-year observations from 1992 to 2000, we find evidence that charities use joint costs to mitigate changes in the program ratio.

Keywords: *charities; nonprofits; joint costs; earnings management.*

Data Availability: *Data are publicly available.*

I. INTRODUCTION

This paper examines whether charities use discretion to alter their reported program ratio—a widely used measure of charity efficiency. While extensive research documents that publicly traded for-profit corporations manage financial results to achieve a number of objectives, little research examines whether nonprofits seek to manage their reported financial results.[1] While incentives and objectives of charities and their management differ from those for commercial firms, financial results for charities can be consequential and, therefore, potentially worth managing.

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[1] Krishnan et al. (2005) examine whether charities use managerial discretion to increase the program ratio. Several other papers examine whether charities manage outcomes for tax and other motivations (Cordes and Weisbrod 1998; Yetman 2001; Leone and Van Horn 2005).

We thank William Baber, Daniel Daly, Patricia Derrick, Mithu Dey, Steve Hansen, Krishna Kumar, Linda Parsons, Debra Sheldon, Billy Soo, Karen Taranto, Kumar Visvanathan, Peter Wilson, Philip Wirtz, and two anonymous reviewers. We gratefully acknowledge the assistance of the Charitable Organizations Division, Office of the Maryland Secretary of State for their time and assistance and Regina Gerrick, Jennifer Kwash, and Mike Park for their data collection efforts. Finally, we thank the American Accounting Association for the financial support provided for this project.

Editor's note: This paper was accepted by Terry Shevlin, Editor.

Submitted January 2004
Accepted August 2005

Charities are required to classify their spending into three categories: programs, fundraising, and administrative activities (FASB 1993; IRS 2002). Business publications, consumer watchdogs, regulators, and umbrella organizations that solicit contributions on behalf of charities provide information on the program ratio—the ratio of program expense to total expense—and identify it as an important consideration for potential donors in deciding whether to give to a particular charity. We test whether charities manage their program ratio to mitigate decreases or increases in the ratio.

Charities have several reasons to avoid reporting decreases in their program ratio. First, empirical evidence finds donation levels are positively associated with reported program ratios (Weisbrod and Dominguez 1986; Posnett and Sandler 1989; Callen 1994; Tinkelman 1999). Second, charities are likely to be subject to unfavorable press and watchdog attention if their program ratio declines substantially or is low, especially if the program ratio falls below watchdog benchmarks (Gold 1993; Schuman 1993; Barrett 1999).[2] Finally, empirical evidence suggests that executive compensation is positively related to changes in the program ratio (Baber et al. 2002).

Charities also have incentives to avoid reporting increases in their program ratio. Lee Cassidy, the then Executive Director of the Nonprofit Federation (a major nonprofit advocacy association), suggests that there are at least two disadvantages of reporting increases in the program ratio. First, if the organization cannot sustain the increase, then management will have to explain a subsequent decrease to donors and its board of directors. Second, the likelihood of regulatory intervention may increase. Regulators may become suspicious of charities with program ratios that are too high or that rise dramatically.[3,4] Because the motivations for mitigating decreases and increases may differ in their importance, we examine whether there is evidence of a differential response to potential increases and decreases in the program ratio.

To study whether charities manage their program ratio, we focus on their allocations of joint costs associated with direct mailings and other activities that combine a public education effort with fundraising appeals.[5] Under the AICPA's Statement of Position (SOP) 87-2 and its successor SOP No. 98-2 (AICPA 1987, 1998), charities must identify joint costs and disclose how they allocate these costs among the program, fundraising, and administrative categories. Changing either the level of joint costs or the share of joint costs allocated to programs can alter the reported program ratio. We study whether charities use either of these methods to manage the program ratio.

Although charities can manage their reported program ratios in other ways, there are several advantages to our focus on joint costs. First, joint costs can be substantial. For the charities in our sample that report joint costs, joint costs are the largest non-programmatic expense—27 percent of total expenses on average, exceeding administrative and fundraising expenses. Second, management has some discretion in calculating and allocating joint costs. For example, charities can increase reported program spending through joint costs by altering mailings to include more program-related and less fundraising-related content or by maintaining the same fundraising strategy but altering the way in which they classify costs. Finally, joint costs have been controversial since they were permitted in 1987 and have been identified as a likely way that charities might manage their reported results. Some charity regulators and watchdog groups criticized SOP No. 87-2 for providing too little guidance and permitting too much flexibility in determining joint-cost allocations (Khalaf 1992; Bush 1992; Davis 1993). In fact, the perception that SOP No. 87-2 permitted too much discretion led to the passage of SOP No. 98-2 in 1998. SOP No. 98-2 provides more specific guidance regarding costs that can be classified as joint, but still allows substantial discretion.

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[2] We cannot test whether charities try to manage the program ratio to meet the benchmarks because so few charities in the sample are in jeopardy of missing the benchmarks.

[3] Mr. Cassidy indicates that this is particularly true for charities that have had problems with ratings organizations or regulators. Sara Lieste, a former Research Associate for Minnesota's Charities Review Council, supports this assertion and suggests that charity regulators tend to become suspicious of charities that report program ratios that are high because a high program ratio may indicate that the charity is misallocating fundraising or administrative expense to programs.

[4] In a different nonprofit context, Leone and Van Horn (2005) find that nonprofit hospitals use earnings management to avoid reporting large profits. They suggest that one motivation for this is to avoid regulatory attention.

[5] Throughout the paper we use joint costs to refer to the shared costs from combining educational and fundraising activities. We do not study other joint costs such as the allocation of overhead or indirect costs. We use the terms costs, expenses and spending interchangeably in this paper. Further, we use joint costs and total joint costs interchangeably.

We test whether charities respond to potential changes in the program ratio by altering the level of joint costs and the share of joint costs allocated to programs. In our empirical analysis, we use a hand-collected dataset of 708 organization-year observations from the 1992–2000 period for charities that report joint costs. We find that charities use both joint-cost levels and the share of joint costs allocated to programs to partially offset potential changes in the reported program ratio. The results indicate that charities use joint costs to offset 16 to 28 percent of potential changes in program ratios. Results provide no evidence that the response to potential decreases and increases in the program ratio differs. We also test whether the behavior of charities differs under SOP No. 87-2 and 98-2 and find no statistically significant evidence of differences.

This research is important for several reasons. It provides evidence that nonprofits, like for-profit firms, manage their accounting results. This finding is important because potential donors use the program ratio to help decide whether to give to a charity and, thus, distortion can lead to misallocation. The ratio is also used by boards of directors in setting pay for charity executives, by watchdog groups in evaluating charities, and state agencies in regulating charities. In addition, the findings support the views of critics of SOP No. 87-2 and SOP No. 98-2 who argue the standards allow considerable discretion.

The next section describes joint cost standards. Section III develops the hypotheses and empirical tests. Data and sample selection procedures are in Section IV. Section V presents the empirical analysis and Section VI concludes.

II. JOINT COST STANDARDS

In 1987, the AICPA issued SOP No. 87-2, *Accounting for Joint Costs of Informational Materials and Activities of Not-For-Profit Organizations That Include a Fund-Raising Appeal.* SOP No. 87-2 applied to costs that were incurred when nonprofits used an activity—such as a direct-mail campaign—to raise money and to inform the public about an issue. For example, if a heart disease prevention charity sends a letter that includes information on steps that the recipient can take to reduce the risk of heart disease and a request for contributions, then the shared costs of the mailing are designated joint costs. For a direct mailing, shared costs typically include postage, labor, mailing list acquisition fees, and printing (Harr et al. 1991; Tinkelman 1998). Under SOP No. 87-2, charities could allocate some of these joint costs to programs if a portion of the activity involved public education and motivated its audience to an action other than providing financial support (AICPA 1987). Prior to the passage of SOP No. 87-2, charities classified all expenses associated with fundraising appeals as fundraising (AICPA 1987).

Although SOP No. 87-2 required that allocation methods be rational, systematic, and applied consistently (given similar facts and circumstances), it neither required nor recommended specific allocation methods.[6] Instead, the AICPA used two examples to define and illustrate cases where it was appropriate to allocate a portion of joint costs to programs. In the first example, a voluntary health and welfare organization described the symptoms of a disease and the actions an individual should take if the symptoms occur. The second example described an organization, whose purpose is to raise public awareness, that alerts individuals to a social or community problem and urges their action in seeking changes (AICPA 1987). Because SOP No. 87-2 defined joint costs through examples, did not provide specific guidance with respect to what kinds of costs were covered, and did not require specific allocation methods, managers had considerable discretion in reporting joint costs. The lack of specific statutory guidance also made it harder for auditors to evaluate "the reasonableness of joint-cost allocations" (Tishlias 1992).

In 1998, the AICPA issued SOP No. 98-2, *Accounting for Costs of Activities of Not-for-Profit Organizations and State and Local Governmental Entities That Include Fund Raising*, effective in fiscal years beginning on or after December 15, 1998. The AICPA revised the 1987 rules because they were difficult to implement and were applied inconsistently in practice (AICPA 1998). The new standard provides more definitive criteria and guidance that define when charities may allocate joint costs. For example, if a charity wants to allocate a portion of the costs to programs, then SOP No. 98-2 requires that mailing recipients cannot be selected "based on the ability or likelihood to contribute to the entity."[7] Another difference is that if a joint activity fails to meet the criteria for allocating a portion of the costs to programs, then all the costs related to the combined activity, including printing of a programmatic message for example, must be assigned to fundraising. However, similar to SOP No. 87-2, the new standard neither requires nor recommends specific allocation methods. Thus, under SOP No. 98-2 managers continue to have discretion with respect to which costs they consider joint and how the joint costs are allocated among programs, fundraising, and administration.[8]

III. HYPOTHESIS DEVELOPMENT AND EMPIRICAL TESTS

Hypotheses

In this section, we discuss our hypotheses and explain our empirical methodology. To facilitate our explanation of the hypotheses and methodology, we introduce terminology related to the cost categories. We separate costs into direct and joint components. Total direct expense is the sum of direct program (*DPE*), direct fundraising, and direct administrative expenses. Total joint costs (*TJC*) is the sum of the joint costs allocated to the program (*JPC*), fundraising, and administration categories. Total program expense (*TPE*) equals the sum of direct program expense and joint program cost.

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[6] See AICPA (1998) and Nitterhouse and Sharp (1987) for suggested allocation methods.

[7] See AICPA (1998) and Yogus (1998) for additional details on differences between SOP No. 87-2 and SOP No. 98-2.

[8] See Billitteri and Blum (1998) for criticisms of SOP No. 98-2. For instance, Bennett Weiner, ex-director of the Council of Better Business Bureau's Philanthropic Advisory Service (which is now the Wise Giving Alliance) states that because the new policy fails to require charities to use a specific allocation method, "differences of opinion as to how much of a campaign should be recognized as a program expense as opposed to a fund-raising cost" may remain (Billitteri and Blum 1998, 42).

$$TPE = DPE + JPC \tag{1}$$

JPC is equal to the product of total joint costs and the share of joint costs assigned to programs. Thus, total program expense equals:

$$TPE = DPE + (TJC \times JPC\%) \tag{2}$$

where *JPC%* = joint costs allocated to programs/total joint costs.

In our analysis, we focus on the total program ratio *TPRatio*, which is equal to *TPE* divided by total expenses. Dividing both sides of Equation (2) by total expenses, *TPRatio* equals:

$$TPRatio = DPRatio + (TJCRatio \times JPC\%) \tag{3}$$

where:

TPRatio = total program expenses/total expenses;
DPRatio = direct program expenses/total expenses; and
TJCRatio = total joint costs/total expenses.

As Equation (3) indicates, the program ratio can change when the direct-program ratio, the joint-cost ratio, (*TJCRatio*) or the share of joint costs allocated to programs (*JPC%*) changes. Therefore, if a charity is faced with an increase (decrease) in the direct-program ratio and it wants to minimize reported changes in the program ratio, the charity can only offset the change by decreasing (increasing) its joint-cost ratio or its joint program share.

There are a number of actions that charities can take to alter the joint-cost ratio or the joint program share. Some of the actions involve making real strategic changes in public education and/or fundraising activities. For instance, a charity that wants to increase the program ratio can include programmatic letters along with a greater number of fundraising mailings (to raise the level of joint costs) or can include more programmatic content per joint mailing (to raise the joint program share). Another option involves making accounting changes. For example, a manager can review all fundraising activities in which the charity engaged and "determine" that some fundraising activities had an educational component or that a larger share of the joint activities involves education.

A charity would only choose, however, to use joint costs to manage program ratios if some users of the information use the program ratio without adjusting for the level of joint program costs included in it. Tinkelman (1998) and Gordon et al. (2005) provide evidence that potential donors use the reported program ratio in making donation decisions without considering the effects of joint costs. Tinkelman (1998) finds that, in the aggregate, donors act as if they do not distinguish between joint costs allocated to programs and direct-program spending in making contributions. Consistent with Tinkelman's (1998) aggregate findings, Gordon et al. (2005), in an experimental context, find that when the program ratio is presented, donors do not utilize information on joint costs allocated to programs.

To determine whether charities use joint cost reporting to mitigate changes in the program ratio, we test the following two hypotheses (stated in the alternative form):

H1: Changes in the joint-cost ratio vary inversely with changes in the direct-program ratio.

H2: Changes in the share of joint costs allocated to programs vary inversely with changes in the direct-program ratio.

Empirical Implementation

To test both hypotheses, we analyze whether managers respond to changes in the direct-program ratio by altering joint-cost allocations to reduce reported changes in the program ratio. Ideally, this analysis would be accomplished by identifying cases when organizations' direct-program ratio changed exogenously and examining how the portion of costs allocated to joint costs and the share of joint costs allocated to programs change in response to these changes. Unfortunately, we can only observe changes in reported spending, not the combination of external factors (such as changes in postage rates or the health of the economy) or internal decisions (such as spending priorities or how to report transactions) that produce the reported ratios and changes in those ratios.

While our analysis is done using the direct-program ratio as the right-hand-side variable, as if the organization was responding to exogenous changes in direct-program spending, we recognize that these changes are not necessarily exogenous. In fact, managerial decisions affect the direct-program ratio. These decisions include those made to manage the reported program ratio.[9] The direct-program ratio can also change as a result of changes in other spending categories. The initial change may have been a change in administrative, fundraising, or joint-cost spending. Whatever the source of changes in the direct-program ratio, management can chose to use discretion related to joint-cost allocations to mitigate the impact of the change on the reported program ratio. Recall from Equation (3) that for a given change in the direct-program ratio, the only way for management to alter the program ratio is to change the size of joint cost or the share of joint costs allocated to programs.

Testing whether charities alter their joint-cost levels to counteract changes in the direct-program ratio (H1) is complicated because the direct program, direct fundraising, direct administration, and joint-cost ratios sum to 1. As a result, a change in any category is associated with changes in the opposite direction in some or all of the other categories. If the direct-program ratio increases by 2 percent, then the sum of the shares of the other three spending categories (i.e., direct fundraising cost, direct administration cost, and joint costs) must decrease by 2 percent and the joint-cost ratio is likely to decrease. Thus, even in the absence of deliberate ratio management, there is a natural negative relationship between changes in the joint-cost ratio and changes in the direct-program ratio. As a result, we cannot merely regress changes in the joint-cost ratio on changes in the direct-program ratio and test whether the coefficient is different from zero because even in the absence of ratio management, the coefficient will tend to be negative.

We use two approaches to consider and adjust for this negative relationship. Both are based on the assumption that in the absence of ratio management, changes in spending on joint activities are proportional to the joint cost's portion of total expenses excluding direct program expense in the previous period. We call the share of nondirect-program spending attributable to joint costs in the previous period *PriorJC%* that equals:

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[9] Charities have discretion in reporting direct program expenses. For example, charities must determine how to allocate the CEO's time across activities. The direct-program ratio that we use in our analysis reflects any of these other efforts at ratio management. The ratio management we study is incremental to any ratio management that alters the direct-program ratio.

$$PriorJC\% = joint\ costs_{t-1}/(joint\ costs_{t-1} + direct\ fundraising\ expense_{t-1} + direct\ administrative\ expense_{t-1}). \quad (4)$$

The assumption that an increase (decrease) in direct-program ratios is accompanied by across-the-board proportional decreases (increases) in the shares of the other cost categories is based on the belief that charities, like other organizations, when faced with the need to adjust spending will tend to preserve existing spending patterns. An example of this is an across-the-board budget cut when resources are limited. In a book on nonprofit budgeting, Maddox (1999) lists across-the-board cuts as one of three common methods of cutting budgets for nonprofits. In addition, an article on budget cuts at state governments reports that "28 states imposed across-the-board cuts" (Ichniowski and Rosta 2003). These sources both suggest that the assumption of across-the-board changes is reasonable.

To provide evidence on the general validity of the assumption of across-the-board proportional changes in the shares of other cost categories in response to changes in program spending, we use the population of nonprofits that file Form 990 and report program, administrative, and fundraising expenses to examine the changes in the administration and fundraising ratios in response to changes in the program ratio.[10] We examine whether the changes in administration and fundraising spending are proportional to their previous shares of non-program spending using the two approaches that we describe below. We find that changes for each spending category deviate little from being proportional to its share of spending in the previous period.[11] This evidence is consistent with our belief that the assumption that changes are proportional to previous spending patterns is a reasonable assumption about what would occur in the absence of ratio management, but our tests are biased to the degree that this assumption is inaccurate. If joint costs change more (less) than other costs for reasons unrelated to ratio management, then this will bias results toward (against) finding ratio management.

We examine the empirical relationship of changes in the joint-cost ratio with changes in direct-program ratio (or in the case of the second approach, a variable calculated using the direct-program ratio) and compare it with the levels we expect in the absence of ratio management. In the first approach, we directly estimate that relationship using the following specification:

$$\Delta TJCRatio = \alpha_0 + \alpha_1\ \Delta DPRatio + \varepsilon. \quad (5)$$

We then test whether α_1 (which should be negative) is statistically different from -1 multiplied by the average *PriorJC%* for the sample (0.58, see Table 2). If charities attempt to stabilize reported program ratios, then we expect α_1 to be more negative than -0.58.

In the second approach, we define a new variable *PredictedEffect* and use it instead of *ΔDPRatio. PredictedEffect* is the product of *ΔDPRatio* and the firm-specific level of the share of non-program spending due to joint costs in the previous period, *PriorJC%*. In the absence of ratio management and under the assumption of proportional changes, this product should equal -1 multiplied by the change in the joint-cost ratio. For example, if 60 percent of costs other than direct-program spending relate to joint costs (i.e., $PriorJC\% = 0.6$) and $\Delta DPRatio$ is 2 percent, then the predicted effect, *PredictedEffect*, is 1.2 percent. If actual changes in total joint costs are proportional to *PriorJC%*, then $\Delta TJCRatio$ is -1.2 percent. We examine the empirical relationship between the change in the joint-cost ratio and *PredictedEffect* using the following equation:

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[10] We study the administrative and fundraising categories for the population of charities because they are the only non-program expenses for most charities. We use a database provided by the National Center of Charitable Statistics (NCCS). The sample consists of 62,726 observations (after removing the extreme 1 percent of the distributions) from 1998–2000.

[11] The largest deviation occurs for the share of spending allocated to fundraising, which changes by 68.5 percent of changes in the direct-program ratio compared to its previous share of non-programmatic spending of 64.0 percent.

$$\Delta TJCRatio = \beta_0 + \beta_1 \, PredictedEffect + \varepsilon \tag{6}$$

where $PredictedEffect = PriorJC\% \times \Delta DPRatio$.

We test whether β_1 equals -1.0, as it would under the assumption of proportional changes and no ratio management. If charities manage their joint costs to mitigate changes, then we expect β_1 will be more negative than -1.0. Although this second approach to testing H1 is more complex, it has the advantage of using charity-specific information on the previous level of *PriorJC%* instead of assuming implicitly that the change for all charities is proportional to the average *PriorJC%*.

In the Appendix, we use simulations to examine our assumptions about the empirical relationships between the components of spending in the absence of ratio management for both tests of HI. The simulations provide evidence that is consistent with our assumptions.

To test whether the proportion of joint costs allocated to programs is associated with direct-program spending (H2), we utilize a simpler empirical approach. This is possible because the share of joint costs allocated to programs has no natural correlation with the direct-program ratio. The joint-cost program share is measured as a proportion of joint costs rather than as a share of all spending. We use the following regression to test H2:

$$\Delta JPC\% = \gamma_0 + \gamma_1 \Delta DPRatio + \varepsilon. \tag{7}$$

If charities do not adjust the share of joint costs allocated to programs in response to changes in direct-program spending, then $\gamma_1 = 0$. If management adjusts the portion of joint costs allocated to programs to reduce the effect of direct-program ratio changes on changes in reported program ratios, then we expect $\gamma_1 < 0$.

IV. DATA AND SAMPLE SELECTION

The sample includes §501(c)(3) charities that engage in fundraising and receive donations. The sample does not include hospitals, schools, pensions, social clubs, local governments, or religious organizations. Secular tax-exempt charities with revenues more than \$100,000 or assets greater than \$250,000 must file Form 990 with the Internal Revenue Service (IRS). We use Form 990 filings—the only publicly available nonprofit financial document—as our source of data. We hand collect Form 990s in our dataset because joint-cost information is not available on machine-readable databases.[12]

Form 990 is prepared on the basis of IRS instructions, which are similar but not identical to financial reporting standards. As a result, it is possible that some charities do not follow GAAP in reporting joint costs on their Form 990.[13] We investigate whether charities follow AICPA rules related to joint costs when preparing their Form 990. Based on our examination of 2001 and 2002 Form 990s and discussions with accountants from our sample charities, we conclude that charities in our sample use SOP No. 98-2 to report and allocate joint costs on Form 990.[14] Although we cannot be certain that the same is true under SOP No. 87-2, the charity accountants whom we spoke to suggest there is no reason not to use the same method for both purposes and none indicated that they had ever used a different method to allocate joint costs for IRS reporting purposes than the method provided in AICPA rules. As a result, we feel comfortable that charities use SOP Nos. 87-2 and 98-2 to allocate joint costs.

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[12] We obtain copies of Form 990 filings by mail requests directly from the charities, from files maintained by the Charitable Organization Division in the Office of the Maryland Secretary of State, and from the IRS.

[13] Research by Froelich and Knoepfle (1996) and Froelich et al. (2000) indicate that accounting and tax filings are generally similar but do differ in some cases. They do not examine joint-cost allocations.

Table 1 summarizes the sample selection process. The initial population of 658 organizations consists of 458 organizations that are evaluated by two charity watchdog groups, the National Charities Information Bureau (NCIB) and the Council of Better Business Bureau's Philanthropic Advisory Service (CBBB-PAS), and 200 charities that are randomly selected from a list of charities registered with the Maryland Charitable Organization Division.[15]

Watchdog groups tend to evaluate large, national organizations. These charities typically solicit donations from individuals who have no personal experience with the charity. Charities on these lists typically rely on direct mail as a method of communicating both program and fundraising information and, therefore, are more likely to report joint-cost information. Almost half (45.5 percent) of these charities (for which we have data) report joint costs. In contrast, only 19 of the other 200 charities (9.5 percent) report joint costs.[16]

The sample is drawn from tax years 1992–2000. We chose the starting date because Form 990 did not separately report joint cost data until 1992. Form 990s covering tax years that begin after December 15, 1998 are subject to SOP No. 98-2.

We eliminate organizations that do not report joint costs. Because the variables are measured as changes, we require that the charity report joint costs on Form 990 for at least two consecutive years. We also eliminate observations when Form 990 contains obvious reporting errors.[17] The final sample consists of 708 observations that represent 155 charities.

V. EMPIRICAL ANALYSIS

Descriptive Statistics

Table 2 displays descriptive information for the sample organizations. Mean (median) total revenue is \$47.1 (\$10.2) million and the mean (median) ratio of contributions to total revenue is 89.1 percent (93.1 percent). Mean (median) total expenses is \$42.1 (\$9.7) million.

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[14] Beginning with the 2001 tax year, Form 990 was amended to include a box that charities are supposed to mark if they use SOP No. 98-2 to account for joint costs in preparing their 990s. The bulk of charities mark the box. For charities that do not mark the box, we contacted the charity's accountant or external auditor to ask them whether they do not follow SOP No. 98-2 or had inadvertently failed to mark the box. In all cases, the charities indicate that they follow SOP No. 98-2 and failed to mark the SOP No. 98-2 box.

[15] The 458 charities are listed in one or more of the February 1996 and June 1997 editions of *Wise Giving Guide* (NCIB 1996, 1997) and the 1996 edition of the *Annual Charity Index* (CBBB-PAS 1996). Subsequently, the NCIB and CBBB-PAS have merged to form the Wise Giving Alliance.

[16] Consistent with these patterns, 40 of 100 large charities that *Worth* (Yaqub 2001) identifies as being "the best" report joint costs, while only 1.8 percent of charities in the NCCS database (that report fundraising spending) report joint costs. The number of charities that report joint costs is relatively small because charities are likely not to report joint costs if (1) public education is not one of their objectives, (2) they do not use direct-mailings, or (3) the charity opts to engage in all public education and fundraising activities separately.

[17] Froelich and Knoepfle (1996) and Froelich et al. (2000) report that Form 990s are generally reliable sources of data, but that there are errors.

TABLE 1
Sample Selection

Panel A: Sample Size

Total 990s requested[a]		658
Request returned—new address unavailable	7	
Organizations not required to file 990 information[b]	69	
No response[c]	55	(131)
Total organizations included in database		527
Organizations that do not report joint costs	355	
Two consecutive years of data unavailable	13	
Mathematical errors on Form 990	4	(372)
Total organizations in sample		155

Panel B: Observations per Organization

Years of Data Used in Regressions	Number of Organizations	Number of Observations	% of Total Observations
One	17	17	2.4
Two	21	42	5.9
Three	22	66	9.3
Four	20	80	11.3
Five	12	60	8.5
Six	25	150	21.2
Seven	15	105	14.8
Eight	19	152	21.5
Nine	4	36	5.1
Totals	155	708	100.0

[a] All organizations listed in the February 1996 and June 1997 editions of the *Wise Giving Guide* and the 1996 edition of the *Annual Charity Index* and 200 randomly selected organizations registered with Charitable Organization Division, Office of the Maryland Secretary of State.

[b] Includes religious organizations; organizations with total receipts less than $100,000; and organizations not registered with the IRS. Also includes 501(c)(4) organizations.

[c] Includes organizations that do not respond to mail requests and are not registered with Charitable Organization Division, Office of the Maryland Secretary of State.

Sample charities are larger and rely more heavily on contributions than the population of charities that file Form 990 and report fundraising expenses.[18]

The charities in our sample have a broad range of charitable objectives. The most common types are health-related and environmental/animal-related organizations that comprise 34 percent and 21 percent of the sample, respectively.

Sample characteristics indicate joint costs are a sizable share of total expenses, so decisions that affect total joint costs and the share of joint costs allocated to programs can have a nontrivial effect on reported ratios. In particular, the mean (median) share of total

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[18] We use a database provided by the National Center of Charitable Statistics (NCCS) to compute the mean revenue and the mean ratio of contributions to revenue for the population of charities that file Form 990. Mean revenue and the mean ratio of contributions to revenue is $5.4 million and 59.7 percent, respectively. The average program ratio for the NCCS charities is 75.5 percent, which is similar to the level for charities in our sample.

TABLE 2
Descriptive Statistics of Sample Characteristics
(n = 708)

Variables	Mean	1st Quartile	Median	3rd Quartile	Standard Deviation
Total revenue (in $M)	47.06	4.13	10.15	34.29	223.60
Ratio of contributions to total revenue	0.891	0.839	0.931	0.971	0.326
Total expense (in $M)	42.14	3.82	9.68	30.49	201.58
Ratio of program expense to total expense *TPRatio*	0.742	0.707	0.761	0.812	0.130
Ratio of direct program expense to total expense *DPRatio*	0.600	0.500	0.664	0.744	0.215
Ratio of joint program costs to total expense	0.142	0.042	0.092	0.203	0.136
Ratio of joint costs to total expense *TJCRatio*	0.267	0.097	0.181	0.381	0.238
Ratio of joint program costs to joint costs *JPC%*	0.536	0.412	0.538	0.680	0.194
Ratio of joint costs to joint costs + direct fundraising expense + direct administrative expense *PriorJC%*	0.578	0.375	0.590	0.811	0.264

All data are from Form 990, Part II. Joint costs are in the "Reporting of Joint Costs" section. Direct program expense equals total program expense less joint program costs.

costs allocated to programs is 74.2 percent (76.1 percent) of which 14.2 percent (9.2 percent) relate to joint costs allocated to programs and the remaining 60.0 percent (66.4 percent) relate to direct program expenses. Mean (median) joint costs as a fraction of total cost is 26.7 percent (18.1 percent), and sample charities allocate more than half (53.6 percent mean and 53.8 percent median) of joint costs to programs.

Table 3, Panel A presents descriptive statistics for the regression variables. All have distributions with means and medians near zero. However, means for the independent variables (Δ*DPRatio* and *PredictedEffect*) are significantly greater than zero (at $p < 0.05$), indicating changes in direct-program ratios are positive on average in the sample period. The mean Δ*TJCRatio* is significant and negative.

Panel B of Table 3 reports correlations of the regression variables. As expected, Δ*DPRatio* and *PredictedEffect* are highly correlated, because we use the former to calculate the latter. Consistent with our expectations, both variables are significantly negatively correlated with Δ*TJCRatio* and Δ*JPC%*. Finally, the two dependent variables are uncorrelated, suggesting that our approach of studying their responses to changes in the direct-program ratio separately, without considering interaction effects, is reasonable.

Primary Results

The primary results are presented in Tables 4 and 5. Because we test whether some coefficients differ from values other than zero, we report standard errors rather than t-statistics. The results reported include the full sample of 708 observations. Results are similar when we remove outliers with large DFFITS values (see Belsley et al. 1980).

The results from the first approach to testing H1, where we compare the estimated relationship between Δ*TJCRatio* and Δ*DPRatio* with the mean proportion of joint costs to

TABLE 3
Descriptive Statistics of Regression Variables
(n = 708)

Panel A: Descriptive Statistics

Variables	Mean	1st Quartile	Median	3rd Quartile	Standard Deviation
ΔDPRatio (independent variable)	0.006	−0.021	0.005	0.032	0.062
PredictedEffect (independent variable)	0.006	−0.011	0.001	0.017	0.044
ΔTJCRatio (dependent variable)	−0.006	−0.028	−0.003	0.018	0.075
ΔJPC% (dependent variable)	−0.001	−0.040	0.000	0.035	0.114

Panel B: Correlations (p-values in parentheses)

Variables	*ΔDPRatio*	*PredictedEffect*	*ΔTJCRatio*
PredictedEffect	0.934 (0.01)	—	
ΔTJCRatio	−0.840 (0.01)	−0.802 (0.01)	—
ΔJPC%	−0.098 (0.01)	−0.081 (0.03)	0.011 (0.77)

Variable definitions:
ΔDPRatio = change in ratio of direct program expense to total expense;
PredictedEffect = ratio of joint costs to the sum of direct fundraising, direct administration, and joint costs multiplied by the change in the ratio of direct program expense to total expense;
ΔTJCRatio = change in ratio of total joint costs to total expense; and
ΔJPC% = change in joint costs allocated to programs divided by joint costs.

non-direct program costs (*PriorJC%*) are reported in Table 4, Panel A. The estimated coefficient on *ΔDPRatio* is −1.00, which is statistically significantly more negative than −0.58 (the sample mean *PriorJC%* × −1.0)—the change we expect under the assumption of no ratio management. In this and our other significance tests we use a 5 percent level to assess statistical significance. The results are consistent with H1. The estimated coefficient is also statistically significantly different from the median and the third quartile of the distribution of *PriorJC%*.

The results from the second approach are presented in Panel B of Table 4. Here we test whether the coefficient on *PredictedEffect* differs from −1.0, the value it would take if changes in joint costs are proportionate to *PriorJC%*. Results presented in the first column of Panel B indicate the coefficient on *PredictedEffect* is −1.37, which is statistically significantly different from −1.0. Thus, results from both approaches used to test H1 indicate that the estimated relationship between changes in joint costs and the direct-program ratio are more negative than would occur if changes in direct-program ratio were proportionately associated with changes in other costs. These results suggest that charities alter the size of joint costs to partially counteract changes in the direct-program ratio.

Results reported in Table 5 are consistent with H2. We find that changes in the share of joint costs allocated to programs are negatively associated with changes in the direct-program ratio. In particular, the estimated coefficient on *ΔDPRatio* is −0.18, which is statistically different from zero. This indicates that charities use the share of joint costs allocated to programs to partially counteract changes in the direct-program ratio. In sum, our basic results indicate that charities shift the portion of expenses allocated to joint costs

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TABLE 4
Regression Estimates of the Change in Joint-Cost Ratio on the Change in the Direct-Program Ratios and on the Predicted Change in Joint-Cost Ratio

Entries are parameter estimates (standard errors)

Variable (predicted sign)	Full Sample (n = 708)	$\Delta DPRatio > 0$ (n = 391)	$\Delta DPRatio < 0$ (n = 317)
Panel A: $\Delta TJCRatio = \alpha_0 + \alpha_1\, \Delta DPRatio + \varepsilon$			
Intercept	0.001 (0.002)*	0.008 (0.003)*	−0.011 (0.003)*
ΔDPRatio (−)	−1.004 (0.024)*	−1.065 (0.040)*	−1.161 (0.052)*
Adjusted R^2	0.705	0.648	0.607
Panel B: $\Delta TJCRatio = \beta_0 + \beta_1\, PredictedEffect + \varepsilon$			
Intercept	0.003 (0.002)	0.002 (0.002)	0.004 (0.004)
PredictedEffect (−)	−1.370 (0.038)*	−1.365 (0.043)*	−1.331 (0.105)*
Adjusted R^2	0.642	0.719	0.337

* Indicates significance (two-tailed) at p-values less than or equal to 0.05. We test whether the coefficient on *DPRatio* is significantly different from 0.578 and whether the coefficient *PredictedEffect* is significantly different from 1.0 on the mean share of *PriorJC%*.

Variable definitions:

ΔDPRatio = change in ratio of direct program expense to total expense;
PredictedEffect = ratio of joint costs to the sum of direct fundraising, direct administration, and joint costs multiplied by the change in the ratio of direct program expense to total expenses; and
ΔTJCRatio = change in ratio of total joint costs to total expenses.

TABLE 5
Regression Estimates of the Change in Joint Program Share on the Change in the Direct-Program Ratios

Entries are parameter estimates (standard errors)

$$\Delta JPC\%_t = \gamma_0 + \gamma_1 \Delta DPRatio_t + \varepsilon$$

Variable (predicted sign)	Full Sample (n = 708)	$\Delta DPRatio > 0$ (n = 391)	$\Delta DPRatio < 0$ (n = 317)
Intercept	0.000 (0.004)	0.000 (0.008)	−0.003 (0.009)
ΔDPRatio (−)	−0.180 (0.068)*	−0.160 (0.119)	−0.238 (0.143)
Adjusted R^2	0.008	0.002	0.006

* Indicates significance (two-tailed) at p-values less than or equal to 0.05. We test whether coefficients are equal to zero.

Variable definitions:

ΔDPRatio = change in the ratio of direct program expense to total expense; and
ΔJPC% = change in the ratio of joint costs allocated to programs to total joint costs.

and the share of joint costs allocated to programs to mitigate potential changes in the program ratio.[19,20]

Because the reasons for mitigating decreases and increases in program ratios differ, charities may respond differently to potential decreases and increases in the program ratio. To test whether responses differ, we partition the sample into observations by whether the direct-program ratio decreases or increases and test whether the coefficients on the variables of interest differ across the two partitions. The results for the regressions are reported in the second and third columns in Tables 4 and 5. None of the differences in coefficients for the variables of interest are statistically significant. This finding suggests that charities have similar responses to potential increases and decreases in their program ratio.

Magnitude of Responses

To consider the economic significance of our results, we quantify the average size of the responses that charities make in reaction to changes in direct-program spending. We first estimate the size of the response of the *TJCRatio*. We compare the estimated coefficient of -1.004 for the Δ*DPRatio* with -0.578 (-1.0 multiplied by mean (*PriorJC%*)—the coefficient we expect if changes are proportional to the previous size of each spending component. The difference is an estimate of the difference between the observed behavior of the charity and the behavior we anticipate if the charity does not manage its ratios. The difference is 0.426 and indicates that for a 1 percent decrease in direct-program spending, joint costs increase by 0.426 percent more than if the charity did not manage its ratios. To estimate the effect of this change on reported program ratios we multiply 0.426 by 0.536, the average share of joint costs allocated to programs (see Table 2). The product, 0.228, indicates that charities alter the *TJCRatio* to counteract 22.8 percent of the change in direct-program spending. A similar calculation for the second approach yields 11.5 percent as an alternative estimate of the degree to which charities alter joint costs to counteract changes in direct-program spending.

We explore the response of the share of joint costs allocated to programs using a similar technique, using the -0.180 coefficient from Table 5. The coefficient indicates that if the direct-program ratio decreases by 1.0 percent, the share of joint costs allocated to programs increases by 0.18 percent. To calculate the effect of this on the reported program ratio, we multiply this figure by 0.267, the average joint-cost ratio. The product is 0.048, which indicates that charities alter the share of joint costs allocated to programs to counteract 4.8 percent of the change in direct-program spending.

Combining this figure with the results from the previous calculations indicates that between 16.3 (11.5 percent + 4.8 percent) and 27.6 percent (22.8 percent + 4.8 percent) of changes in direct-program spending are counteracted by altering the size and allocation of joint costs. In our sample, the average change in the absolute value of the direct-program ratio is 4.18 percent. These numbers indicate that, on average, charities' joint-cost allocations alter their reported program ratios by 0.68 (0.163 × 4.18) percent to 1.15 (0.276

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[19] To control for possible year effects, we repeat our analysis including year-dummy variables. The year dummies have no effect on the variables of interest and are never statistically significant.

[20] We explore whether the findings of ratio management hold across charitable objectives by partitioning the sample into four industry categories and separately running Equations (5) through (7) for each category. In particular, we sort charities into four broad categories: (1) medical, (2) social services, (3) environmental and animal, and (4) other. We find that the basic results are similar to those reported in Tables 4 and 5 except that the coefficient on Δ*DPRatio* is not significant in tests of H2 for social services and medical charities. As in the full sample, none of the differences in the estimated coefficients are statistically significantly different between the increase and decrease subsamples.

× 4.18) percent. This suggests that ratio management related to altering joint-cost allocations is fairly substantial.

Comparison of Results under SOP No. 87-2 and SOP No. 98-2

We investigate whether the general finding that charities manage their program ratio using their joint-cost allocations extends to the periods under SOP No. 87-2 and SOP No. 98-2. The new rule, effective for financial statements for fiscal years that begin after December 15, 1998, is intended to provide clearer guidance related to joint costs. It is possible managers have less flexibility to manage ratios under SOP No. 98-2. Roberts (2005) finds that charities report lower joint costs under SOP No. 98-2.

In Tables 6 and 7, we report results from regressions that study the subperiods separately. In Table 6, we report results from both approaches that examine whether charities alter the size of their joint costs in response to changes in the direct-program share. In Table 7, we examine the relationship between the share of joint costs allocated to programs and direct-program share. The results are presented in the same format in the two tables. In the first column, we repeat the results for the full sample. In the second column, we report results for the 445 observations in the period covered by SOP No. 87-2. The final column presents the results for the 263 observations under SOP No. 98-2.

The results reported in Table 6, Panel A, indicate that the response of *ΔTJCRatio* to *ΔDPRatio* is similar for the two periods (−0.99 and −1.05 for SOP No. 87-2 and SOP No.

TABLE 6

Regression Estimates of the Change in Joint-Cost Ratio on the Change in the Direct-Program Ratios and on the Predicted Change in Joint-Cost Ratio

Entries are parameter estimates (standard errors)

Variable (predicted sign)	Full Sample (n = 708)	SOP No. 87-2 Sample (n = 445)	SOP No. 98-2 Sample (n = 263)
Panel A: $\Delta TJCRatio = \alpha_0 + \alpha_1 \Delta DPRatio + \varepsilon$			
Intercept	0.000	0.002	−0.002
	(0.002)	(0.002)	(0.002)
ΔDPRatio (−)	−1.004	−0.985	−1.046
	(0.024)*	(0.030)*	(0.041)*
Adjusted R^2	0.705	0.704	0.710
Panel B: $\Delta TJCRatio = \beta_0 + \beta_1 PredictedEffect + \varepsilon$			
Intercept	0.003	0.004	0.000
	(0.002)	(0.002)	(0.002)
PredictedEffect (−)	−1.370	−1.353	−1.404
	(0.038)*	(0.050)*	(0.059)*
Adjusted R^2	0.642	0.622	0.684

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* Indicates significance (two-tailed) at p-values less than or equal to 0.05. We test whether the coefficient on *PredictedEffect* is significantly different from 1.0 and whether the coefficient on *DPRatio* is significantly different from to 0.578, the mean share of *PriorJC%*.

Variable definitions:

ΔDPRatio = change in ratio of direct program expense to total expense;

PredictedEffect = ratio of joint costs to the sum of direct fundraising, direct administration, and joint costs multiplied by the change in the ratio of direct program expense to total expenses; and

ΔTJCRatio = change in ratio of total joint costs to total expenses.

TABLE 7
Regression Estimates of the Change in Joint Program Share on the Change in the Direct-Program Ratios

Entries are parameter estimates (standard errors)

$$\Delta JPC\%_t = \gamma_0 + \gamma_1 \Delta DPRatio_t + \varepsilon$$

Variable (Predicted Sign)	Full Sample (n = 708)	SOP No. 87-2 Sample (n = 445)	SOP No. 98-2 Sample (n = 263)
Intercept	0.000 (0.004)	0.005 (0.005)	−0.008 (0.007)
ΔDPRatio (−)	−0.170 (0.068)*	−0.193 (0.078)*	−0.143 (0.134)
Adjusted R^2	0.008	0.011	0.001

* Indicates significance (two-tailed) at p-values less than or equal to 0.05. We test whether coefficients are equal to zero.

Variable definitions:

ΔDPRatio = change in the ratio of direct program expenses to total expenses; and

ΔJPC% = change in the ratio of joint costs allocated to programs to total joint costs.

98-2 periods, respectively). The results in Panel B are also similar across the two periods. None of the differences in estimated effects are statistically significantly different for the two subperiods.[21] In both periods, charities use the size of joint costs to reduce changes in their program ratios.[22]

In Table 7 the estimated coefficient on the *ΔDPRatio* (−0.19) is significant in the SOP No. 87-2 period. In the SOP No. 98-2 period the estimated effect of *ΔDPRatio* is smaller (−0.14) and is not significantly different from zero.[23] The difference between the estimated coefficients for the two subperiods is not statistically significant.[24]

The finding in Table 7 that the coefficient on *ΔDPRatio* for the SOP No. 98-2 period is not statistically significantly different from both zero and the coefficient in the earlier period makes it impossible to confidently conclude whether the new rule had an impact. The lack of statistical power due to the small sample size of only 263 observations in the SOP No. 98-2 period and any noise due to charities attempting to understand the new rule may contribute to the inconclusive result.

Overall the results indicate that the pattern of charities using their joint costs to reduce the impact of potential changes in their program ratio prevails both under SOP No. 87-2 and SOP No. 98-2. In both periods, charities use the portion of costs allocated to joint costs to manage their program ratio. The evidence for the use of allocations of joint costs between program and other cost categories is mixed with evidence of ratio management in the earlier period and inconclusive results for the later period.

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[21] We repeat the SOP No. 98-2 analysis excluding observations in the first year under the new rule. We examine this sample because the variables in the specifications are measured as changes and the previous period is under the earlier rule. Results for that analysis are similar.

[22] The pattern of similar results for the subperiods also prevails for the increase and decrease in direct-program spending subsamples.

[23] The coefficient is closer to zero when the first year under SOP No. 98-2 is excluded.

[24] The result that the estimated effects are not statistically significantly different also prevails for the increase and decrease subsamples.

Sensitivity Analyses

We conduct additional tests to evaluate the robustness of our primary results. We consider whether our results are sensitive to mean reversion in joint-cost allocations and to the removal of observations where there is reason to suspect joint costs might change for reasons other than attempts to mitigate reporting changes in the program ratio.

To examine whether the dependent variables are related to the *TJCRatio* (*JPC%*) in the previous period, we include *TJCRatio* at period $t-1$ (*JPC%* at period $t-1$) as an additional explanatory variable in Equations (5) and (6) (Equation (7)). Although there is evidence of mean reversion (especially for *JPC%*) including these variables does not alter the primary results. In particular, the coefficients (standard errors) on Δ*DPRatio* and *PredictedEffect* are −0.993 (0.025) and −1.356 (0.039) when *TJCRatio* at period $t-1$ is included in Equation (5) and Equation (6), respectively. The coefficient on Δ*DPRatio* is −0.168 (0.066) when *JPC%* at period $t-1$ is included in Equation (7).[25] The results are consistent with H1 and H2.

Next, we use two methods to identify observations where changes in joint costs might occur for reasons other than charities managing joint costs to mitigate changes in program ratios. In each case, we repeat our analysis after removing observations where other motivations may dominate.

In the first method, we eliminate observations where the effect on the total program ratio of changes in the *TJCRatio* or *JPC%* is larger than changes in the direct-program ratio.[26] We remove these observations because for these observations the presumption that joint cost changes are made to reduce the changes in the total program ratio is less reasonable. We remove 227 observations from the initial sample and re-estimate Equations (5) through (7).[27] Results are similar to those reported in Tables 4 and 5. In particular, the coefficients (standard errors) on the right-hand-side variables are −0.896 (0.020), −1.274 (0.027), and −0.172 (0.055) for Equations (5), (6), and (7), respectively. The results are consistent with H1 and H2.

In the second method, we identify cases where the presence of other substantial changes may be the major source of changes in reported ratios. In these instances, charities may still use joint costs to try to mitigate changes in the reported program ratio, but relationships among the variables may be largely determined by other factors related to the substantial changes. As a result, including these observations in the sample may distort the results. We identify cases where there are substantial shifts in either overall spending levels or in the share of total spending due to each of three variables related to fundraising—professional fundraiser fees, mailing costs, and printing and publication costs—as observations to remove. We consider changes in overall spending because a charity that experiences a large decrease or increase in its overall spending may be likely to change its spending patterns as part of that shift. Shifts in mailing and printing and publication costs (cost often incurred as a result of direct mail solicitations) and professional fundraiser fees may reflect shifts in fundraising strategies.

We exclude observations in the upper and lower 2 percent of the distributions for each of the four variables and re-estimate Equations (5) through (7). For the change in total spending, this removes observations with growth that is greater (less) than an 83.9 percent increase (34.1 percent decrease). For the other three variables this process removes observations with changes that exceed 5 to 9 percent in absolute value, depending on the variable. This analysis excludes 89 observations. Results are similar to those reported in Tables 4 and 5. In particular, the coefficients (standard errors) on the right-hand-side variables are −0.994 (0.027), −1.474 (0.049), and −0.193 (0.081) for Equations (5), (6), and (7), respectively. The findings are consistent with H1 and H2.

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[25] For this and other sensitivity analyses, we also test whether there is a difference in the responses for the increase and decrease subsamples. As in the primary analysis, we find no statistically significant differences.

[26] To estimate the effect of changes in *TJCRatio* on the total program ratio requires multiplying the change in the joint-cost ratio by the share of joint costs allocated to programs at period $t-1$. Similarly, to estimate the impact of changes in *JPC%*, we multiply the change in the joint program share by the joint costs in $t-1$.

[27] A large number of observations are removed because in many cases all of the changes being compared are small. We obtain similar results when we only eliminate observations with larger changes.

VI. CONCLUSIONS

Among the charities reporting joint costs, the evidence is consistent with these charities using both the level and the percent of joint costs allocated to program expenses to mitigate changes in the program ratio. These actions substantially affect reported ratios, with our estimates indicating that between 16 percent and 28 percent of changes in direct programs are counteracted by changes in ratio of all spending related to joint costs and the share of joint costs allocated to programs. We find no evidence that charities are more averse to reporting decreases in the program ratio than to reporting increases in the program ratio. We examine whether the response of charities to changes in the program share differ under SOP No. 87-2 and SOP No. 98-2 and find no significant differences. The results are robust to several sensitivity checks.

Our results indicate that charitable organizations, like publicly traded corporations, use discretion to manage their reported results. Other research documents several motivations for managing reported results. Cordes and Weisbrod (1998) and Yetman (2001) document that charities use discretion to avoid paying taxes on their for-profit subsidiaries. As we do in this paper, Leone and Van Horn (2005) and Krishnan et al. (2005) examine whether charities manage their financial results to achieve reporting objectives. Leone and Van Horn (2005) provide evidence that nonprofit hospitals manage their results to report small positive profits and to reduce changes in earnings.

Krishnan et al. (2005) examine whether charities manage their reported program ratio to avoid reporting low program ratios. They find evidence that many charities report zero fundraising costs in spite of engaging in fundraising activities. They also document that nonprofit hospitals in California report greater program spending on their Form 990s than they do in state regulatory filings that are audited by the state government. Krishnan et al. (2005) and our paper both provide evidence that charities manage their reported program ratios. Their paper provides evidence that charities take steps to raise their program ratio, while we find evidence that charities try to mitigate decreases and increases in their program ratios.

As is the case for publicly traded corporations, when charities manage their results it can lead to an inefficient use of resources. Some efforts to alter joint-cost allocations may involve nonprofits using internal resources in ways that would otherwise not be optimal. The managed ratios may also lead to donors misallocating their contributions to charities that they would opt not to fund if they had access to information on the unmanaged ratios.

Our findings lend support to critics of SOP No. 87-2 and SOP No. 98-2 who argue that the standards allow substantial discretion to charities in determining joint costs and that this discretion could be used to manage reported results.

APPENDIX

This appendix examines the validity of our assumptions about the empirical association of components of spending in the absence of ratio management that underlie our tests of

H1. To accomplish this we use simulations that introduce random shocks to spending on each category. The results provide support for our empirical approach.

To illustrate the simulations, we consider the following simplified example.[28] In period $t-1$, direct program expense is 60 percent of total expense; joint costs are 20 percent of total expense; and the remaining 20 percent of expenses is allocated to direct fundraising and direct administration expenses. In this example, the initial share of spending on non-direct program categories that relate to joint costs (*PriorJC%*) is 50 percent [0.20/(0.20 + 0.20)]. Based on our arguments in the paper, for this expense allocation the coefficient on Δ*DPRatio* should equal -0.5 and the coefficient on *PredictedEffect* should equal -1.0. We run 1,000 simulations to examine whether the observed regression coefficients differ from these predicted levels.

In each simulation, we generate 11 additional years of data for each of three spending categories: direct program expense, joint costs, and the combination of direct fundraising and direct administration expenses. The simulations assume that charities grow 10 percent per year on average and that each of the expense categories are subject to random shocks each year. For each 11-year simulation, we run the regressions in Table 4, which regress Δ*TJCRatio* separately on Δ*DPRatio* and on *PredictedEffect*. The mean regression coefficients across the 1,000 regressions are -0.49953 and -1.00094, which are close to the theoretical values of -0.5 and -1.0.

The results from these 1,000 simulations support our assertions about the regression coefficients that occur in the absence of ratio management. The results generalize to simulations with other initial spending shares and those that do not combine direct fundraising and direct administration into a single spending category.

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THE ACCOUNTING REVIEW
Vol. 81, No. 1
2006
pp. 179–206

Fundamentals of Accounting Losses

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ABSTRACT: This paper examines accounting and nonaccounting factors behind accounting losses over a 50-year period. Using multivariate time-series analysis, we report evidence that the annual percentage of losses for U.S. firms is significantly related to accounting conservatism, Compustat coverage of small firms, real firm performance as measured by cash flows from operations, and business cycle factors. We further find that nonaccounting factors tend to play the dominant role in explaining accounting losses over our sample period. Our results are robust to alternative definitions of macroeconomic productivity, as well as to varying model specifications. Our findings contribute to the literature on accounting losses and accounting conservatism and have implications for the use of accounting loss information in numerous settings.

Keywords: *accounting losses; accounting conservatism; business cycle; small firms.*

Data Availability: *All data employed in this study are commercially available from the sources described in the text.*

I. INTRODUCTION

Over the last 50 years, as the proportion of firms reporting negative income has increased, many studies have examined various aspects of accounting losses. One strand of research investigates how losses relate to various outcomes, such as security valuation (Collins et al. 1997; Barth et al. 1998; Joos and Plesko 2005), bankruptcy (Barth et al. 1998), and the abandonment option (Hayn 1995; Joos and Plesko 2005). These studies find that non-earnings information, most prominently the book value of equity, takes on a higher degree of relevance for loss firms than for profit firms. Another line of research examines accounting properties associated with negative income. For example, Givoly and Hayn (2000) demonstrate a fall in the return on assets (ROA) over time and attribute this phenomenon to an increase in accounting conservatism, as reflected by nonoperating accruals. Joos and Plesko (2005) show that investors can assess the likelihood that a firm's negative income will persist (or reverse) over time by examining cash flow and accrual components of losses.

We thank Sudipta Basu, John Bildersee, Dan Gode, Carla Hayn, Lee-Seok Hwang, Doug Skinner, Ross Watts, Christine Wiedman, and participants at the accounting workshop at New York University and the AAA Annual Meeting in Atlanta for their valuable comments and suggestions.

Editor's note: This paper was accepted by Patricia Dechow, editor.

Submitted February 2005
Accepted August 2005

The purpose of our study is to investigate the incremental roles that several fundamental nonaccounting factors play in generating accounting losses over and beyond accounting conservatism. Using aggregated time-series data over a 50-year sample period, 1951–2001, we first show a significantly positive temporal association between accounting losses and accounting conservatism, a result consistent with Givoly and Hayn (2000). We build on these results by adding firm size, real performance as measured using cash flows from operations (CFO), and business cycle effects to a multivariate regression of the frequency of accounting losses on conservatism and these additional factors. Our main findings are twofold. First, after controlling for accounting conservatism, the frequency of accounting losses is positively related to the proportion of small firms reported on Compustat and negatively related to firms' CFO and to macroeconomic productivity. Second, while accounting conservatism remains a significant determinant of accounting losses, its overall contribution is lessened considerably when placed alongside the other independent variables. This effect is especially magnified when, as a sensitivity test, we replace loss frequency with median ROA and repeat our analysis; here we find that only the nonaccounting factors are significant determinants of profitability levels over time. Overall, our results suggest that while both accounting and nonaccounting factors are related systematically to accounting losses, nonaccounting factors tend to play the dominant role.

In additional sensitivity tests, we use alternative measures of accounting conservatism and the business cycle in our regression analyses. Our nonaccounting determinants remain robust to whether we use nonoperating accruals, an earnings-return metric (Basu 1997), or the market-to-book ratio (Beaver and Ryan 2000) as a proxy for accounting conservatism. However, we find that only nonoperating accruals and the earnings-return measures are significantly related to the frequency of losses, a finding consistent with the growing literature showing differences among accounting conservatism measures (Watts 2003; Givoly et al. 2004; Roychowdhury and Watts 2004). To capture different aspects and timing of the business cycle, we use three measures—the yearly change in real gross domestic product (ΔGDP), the yearly change in industrial production (ΔIP,) and the National Bureau of Economic Research's (NBER) definition of an expansion or recession. Our results are robust to these alternative measures of macroeconomic productivity.

We also control for time-series trends in the data in two ways. First, we take differences of our model inputs and find that year-to-year changes in the percentage of loss firms are significantly related to changes in the Compustat coverage of small firms, changes in real firm performance, and growth in macroeconomic productivity. In contrast, we find no evidence that changes in the degree of accounting conservatism are associated with changes in loss frequency. Next, we add a trend variable to our levels model and find that our inferences on the nonaccounting variables are unchanged.

Last, we perform additional analysis to explore why small firms report more losses. We provide evidence that small firms are less diversified, have more idiosyncratic risk, invest more, and are more likely to fail than larger firms. We further find that these firm characteristics are similarly related to loss firms *vis-à-vis* profit firms. We conclude that the economic fundamentals of small firms contribute to their likelihood of realizing accounting losses over time.

Our study contributes to the literature in several ways. We expand on existing accounting research that examines determinants behind losses by shedding light on how nonaccounting factors are associated with negative income, after controlling for accounting conservatism. Our evidence suggests that nonaccounting factors, and firm size in particular, play the dominant role over accounting conservatism in determining losses.

We expand on our result on firm size by exploring reasons why small firms are more likely to report losses than are larger firms. Specifically, we show that economic factors intrinsic to small firms make it more likely for these firms to report losses than larger firms. As such, we extend recent findings by Fama and French (2001, 2004), who report that smaller firms generally report lower earnings than larger firms during their sample periods.

We extend the prior literature on analyzing losses by using annually aggregated, market-wide data in our main empirical tests rather than firm-specific data. There are several advantages to taking this approach. It allows us to relate macroeconomic variables to losses—in particular, our business cycle variables are time-dependent macroeconomic variables. In addition, aggregating variables annually provides insights into the long-term time trends of our variables, while firm-specific variables do not lend themselves easily to this task. Aggregation also filters out idiosyncratic components of information that are uncorrelated across firms. For example, Kothari et al. (2003) find that aggregating earnings over time minimizes the effects of noise in firms' earnings. Skinner (2004) makes a similar argument for the relation between dividends and earnings.

Finally, accounting income, and losses in particular, are relevant in determining security valuations, bankruptcy probabilities, and abandonment options. Accounting earnings and losses are inputs in contracting, shareholder litigation, dividend policy, market listing standards, and regulatory inquiries. Our study implies that focusing on accounting conservatism alone could result in poor decision-making.

To illustrate, Hayn (1995) and Joos and Plesko (2005) examine how accounting data influence strings of losses, which, in turn, allow managers to better evaluate its firm abandonment option. By discerning nonaccounting determinants of losses, managers can better assess the permanence and risks inherent to an observed loss and decide in a timelier manner if and when to cease operations. Similarly, security valuation models using net income or changes in net income as independent variables could encompass nonaccounting determinants of losses as additional valuation factors for loss firms.

With regard to contracting, Dichev and Skinner (2002) find that net income frequently is used in accounting-based debt covenants, either directly through a debt to "cash flow" ratio, where cash flow can be cash flows from operations, EBIT, or EBITDA, or indirectly through net worth, where net worth is diminished by accounting losses. They also show that while 30 percent of all loans are in technical default, lenders appear to weigh whether violators are in financial distress, and often waive the violations for financially healthy firms. Since most technical defaulters have negative income (Dichev and Skinner 2002; DeFond and Jiambalvo 1994), understanding the fundamentals behind losses would assist lenders in determining when to call the loan. Likewise, boards of directors can construct executive compensation contracts to minimize the effects of macroeconomic factors as it relates to accounting-based bonuses.

The remainder of the paper is organized as follows. We present our hypotheses linking accounting losses to accounting and nonaccounting factors in Section II and our regression framework in Section III. We describe our data in Section IV and present main results in Section V. In Section VI, we discuss the results of our robustness tests. In Section VII, we perform additional analysis examining why small firms report more losses. Finally, in Section VIII, we summarize and conclude the paper.

II. HYPOTHESES

Our study focuses on the associations between accounting losses and nonaccounting factors after controlling for accounting conservatism. In this section, we discuss hypotheses linking accounting losses to three nonaccounting factors: Compustat coverage of small firms, real performance as measured by cash flows from operations, and macroeconomic productivity. We begin by discussing the already established link between accounting losses and accounting conservatism.

Accounting Conservatism

Based on Givoly and Hayn's (2000) findings, we expect a positive relation between the frequency of accounting losses and accounting conservatism. Givoly and Hayn (2000) show a decline in ROA and a subsequent increase in nonoperating accruals over time for a constant sample of 896 Compustat firms between 1951 and 1998. Their results, along with no marked deterioration in CFO (scaled by total assets) for the same sample, support the hypothesis that the observed decrease in accounting income over time mirrors an increase in accounting conservatism over the same time period.

There are several differences between our study and Givoly and Hayn (2000). One primary distinction is that, in contrast to their study, we use the nearly universal Compustat database. Other differences are that they use firm-specific data and analyze ROA, whereas we utilize an aggregated approach to study the annual frequency of accounting losses.

Compustat Coverage of Small Firms

We predict a positive association between the annual percentage of losses and the rise in the percentage of small firms appearing on the Compustat database over time. Our prediction is consistent with Fama and French (2001, 2004), who report a temporal increase in the percentage of small firms appearing on both the Compustat and CRSP databases, as well as the fact that smaller firms generally report lower earnings than larger firms over their sample period.

There are several possible reasons why small firms are more likely to report losses than larger firms. Many small firms are in the earlier stages of development and therefore are less likely to be reporting positive earnings. Small firms have more volatility in their operations due to being less diversified, being more dependent on fewer customers and/or suppliers, and undertaking more risky investments. Thus, they may alternate between reporting positive earnings and losses. Many small firms are failing companies and therefore are more likely to report losses than larger firms. Thus, we posit that small firms are riskier than larger firms, which, in turn, results in higher incidences of accounting losses over time. We provide additional analysis of why small firms are more likely to report losses in Section VII.

Business Cycle and Macroeconomic Productivity

We predict a negative relation between the frequency of accounting losses and macroeconomic productivity.[1] Business cycles generally are divided into expansions, representing "high" growth rates in economic productivity, and recessions or contractions, which are declines in economic productivity levels.[2] Although there are several measures of macroeconomic productivity, two commonly used statistics are the gross domestic product (GDP) and industrial production (IP).

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[1] Examination of links between accounting earnings and macroeconomic factors has been relatively unexplored in the accounting and economic literature. Johnson (1999) documents that earnings response coefficients (ERC) and earnings persistence are related to business cycles. Stern (1955) finds a "fairly close correlation" between contemporaneous industrial production and "money profits in industry" in the United Kingdom and the United States for the years 1919–1950. Chant (1980) reports a positive relation between leading money supply (M1) and current accounting earnings for 1958–1977; Levi (1980) finds a similar association between the change in M1 and the level of accounting earnings for 1949–1975. Chant (1980), however, finds no relation between bank loans and accounting earnings, and Alessi (1964) reports none between inflation and accounting earnings.

As Hall (1990) shows, there are vast differences in economic variables during expansions and recessions. Using data from 1919 through 1982, he finds that over all expansions, average real GDP rises by 6.0 percent, average real consumption rises by 4.3 percent, and average real investment rises by 27.7 percent. In contrast, the average changes over all recessions are −5.4 percent for real GDP, −0.5 percent for real consumption, and −27.8 percent for real investments. These statistics suggest that firms invest more and that consumers spend more during periods of high macroeconomic productivity and, conversely, firms and consumers cut expenditures during periods of low or negative macroeconomic productivity. We predict that these patterns of expenditures will result in higher frequencies of accounting losses during recessions or periods of low economic productivity.

Cash Flows from Operations

We predict a negative association between the frequency of accounting losses and aggregate cash flows from operations (CFO). CFO includes cash transactions involved in the firm's delivering or producing goods or providing services and, therefore, we view it as a measure of the firm's real performance. In Section V, we present evidence consistent with this view.

III. REGRESSION FRAMEWORK

Unlike most accounting studies that use firm-specific data, we adopt a market-wide approach, regressing the annual percentage of accounting losses for the 1951–2001 period on four aggregated accounting and nonaccounting variables.[3] The basic regression is:

$$\%LOSSES_t = \beta_0 + \beta_1 CNSV_t + \beta_2 SMALL_t + \beta_3 BUSCYC_t + \beta_4 CFOA_t + \varepsilon_t \qquad (1)$$

where:

$\%LOSSES_t$ = the percentage of Compustat firms reporting accounting losses for year *t*;
$CNSV_t$ = a measure of accounting conservatism for year *t*;
$SMALL_t$ = the measure of the relative number of small firms in the Compustat database for year *t*;
$BUSCYC_t$ = a measure of macroeconomic productivity for year *t*; and
$CFOA_t$ = mean cash flows from operations divided by assets for year *t*.

As described in Section II, we choose these independent variables because they measure potentially significant aspects of firms' overall earnings processes. In addition, each can be delineated with relative unambiguity as either an accounting or nonaccounting factor. For example, *CNSV* is clearly an accounting measure in that its magnitude and direction depends greatly on accounting rules over time. *SMALL* and *BUSCYC* basically are unrelated to accounting conservatism—*SMALL* depends on total assets and *BUSCYC* measures the productivity of the overall U.S. economy. *CFOA* is intended to measure real firm performance, a "nonaccounting" metric. Even though CFO is an accounting variable, subject to accounting rules, it is relatively less susceptible to manipulation than accounting earnings or accruals (Dechow 1994; Dechow and Schrand 2004). We discuss the associations among our independent variables later in the paper to assess the classification of these independent variables as "accounting" or "nonaccounting" factors.

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[2] A full business cycle covers a recession and subsequent expansion. Zarnowitz (1985) defines a full cycle from the trough of one contraction to the trough of the next contraction. He identifies ten business cycles from 1933 through 1982 and 30 business cycles from 1854 through 1982.

[3] This approach is widely used in the finance literature to examine market-wide trends in market returns (Fama and French 1992); market volatility (Campbell et al. 2001); the equity premium (Fama and French 2002; Claus and Thomas 2001); dividends (Fama and French 2001); and new listings (Fama and French 2004). Two accounting papers that use aggregated data across time are Skinner (2004), who relates aggregated dividends and earnings, and Kothari et al. (2003), who examine the relation between aggregated earnings and returns.

IV. SAMPLE SELECTION, VARIABLE DEFINITIONS, AND DATA DESCRIPTION

Sample Selection

The sample consists of all firm-years for which net income (Compustat item no. 172) and total assets (Compustat item no. 6) are available on the merged Compustat annual industrial file, including PST, full coverage, and research files. This results in 259,116 observations over the period 1951–2001. The total number of firms varies from a low of 614 in 1951 to a high of 10,313 in 1996.

Table 1 presents the annual frequency of losses. The percentages vary from 0.81 percent in 1951 to 45.86 percent in 2001. Consistent with Hayn (1995), Collins et al. (1997), and Collins et al. (1999), we find that the frequency of losses has risen steadily over time. Frequency rates for each decade are 3.44 percent for 1951–1960, 7.28 percent for 1961–1970, 13.95 percent for 1971–1980, 30.09 percent for 1981–1990, and 35.96 percent for 1991–2001.

Accounting Conservatism

We use the same accounting conservatism measure as Givoly and Hayn (2000). The firm's nonoperating accruals (*NOPACC*) are defined as total accruals (net income minus cash flows from operations) plus depreciation and amortization minus operating accruals (Δaccounts receivable + Δinventories + Δprepaid expenses − Δaccounts payable − Δtaxes payable) divided by beginning period total assets. Removal of these items from total accruals leaves us primarily with gains and losses from nonoperating assets, bad debt expense, restructuring charges, write-downs of assets, deferred income taxes, write-offs of in-process R&D expenses, and other expenses or revenues not included in the operating accrual accounts. Since most nonoperating accruals are expenses or losses, we equate accounting conservatism of the income statement with larger negative amounts.

We choose this measure initially over the Basu (1997) earnings-returns and Beaver and Ryan's (2000) book-to-market value of equity measures for two main reasons. First, *NOPACC* is based on accounting data only. In contrast, the other two measures are market-dependent. Second, several studies (e.g., Dietrich et al. 2003; Givoly et al. 2004) have questioned the reliability of the earnings-return measure as a proxy for accounting conservatism. In fact, Givoly et al. (2004) present evidence of a negative relation between the earnings-return measure and alternative accounting conservatism measures and point to measurement error in the earnings-return measure as a prime reason for these observed observations. In Section VI, we present sensitivity tests in which we use the earnings-returns and book-to-market measures to examine differences among these definitions of accounting conservatism.

TABLE 1
Frequency of Accounting Losses by Year

Year	Total Number of Firms	% of Losses	Year	Total Number of Firms	% of Losses
1951	614	0.81	1979	5,875	13.14
1952	625	1.76	1980	5,988	16.35
1953	636	1.42	1981	6,076	19.55
1954	653	2.91	1982	6,327	25.51
1955	674	1.78	1983	6,604	25.92
1956	692	2.17	1984	6,658	26.65
1957	712	1.97	1985	6,940	31.43
1958	735	3.54	1986	7,256	33.13
1959	763	2.23	1987	7,350	33.33
1960	1,372	9.40	1988	7,250	32.63
1961	1,699	8.18	1989	7,176	34.56
1962	1,918	6.20	1990	7,203	35.37
1963	2,169	6.32	1991	7,366	36.32
1964	2,334	5.36	1992	7,798	34.87
1965	2,489	4.06	1993	8,901	32.00
1966	2,666	4.01	1994	9,310	28.94
1967	2,846	4.88	1995	10,108	31.48
1968	3,430	5.80	1996	10,313	32.14
1969	3,634	8.91	1997	10,056	33.99
1970	3,704	15.31	1998	10,303	39.25
1971	3,895	13.71	1999	10,257	40.13
1972	4,076	8.88	2000	9,531	41.75
1973	4,467	8.46	2001	7,967	45.86
1974	5,885	16.86	1951–1960	7,476	3.44
1975	5,917	18.34	1961–1970	26,889	7.28
1976	5,953	14.53	1971–1980	54,001	13.95
1977	6,009	13.91	1981–1990	68,840	30.09
1978	5,936	12.37	1991–2001	101,910	35.96
			All years	259,116	25.84

Accounting losses are negative values of net income (Compustat item no. 172).

Table 2, Panel A shows the mean and median values of *NOPACC* by decade. *NOPACC* declines in the 1960s, remains relatively constant in the 1970s, and falls dramatically in the 1980s and 1990s. The large negative growth in *NOPACC* in the latter period parallels that presented by Givoly and Hayn (2000) for their constant sample of 896 firms, ending in 1998. Thus, while not monotonic, there nevertheless is a steady rise in accounting conservatism for the full Compustat sample over our time period.

Compustat Coverage of Small Firms

We estimate *SMALL* by comparing the total assets of each firm to the population of NYSE firms (see Fama and French 2001, 2004). For each year, we rank all NYSE firms

TABLE 2
Descriptive Statistics

Panel A: Accounting Conservatism, Percentage of Small Firms, ΔGDP, ΔIP, and CFO By Decade

Time Period	*NOPACC* Mean	*NOPACC* Median	*SMALL* Mean	*SMALL* Median	ΔGDP Mean	ΔGDP Median	ΔIP Mean	ΔIP Median	*CFOA* Mean	*CFOA* Median
1951–1960	−0.011	−0.007	0.285	0.274	0.035	0.032	0.028	0.035	0.101	0.104
1961–1970	−0.015	−0.018	0.452	0.438	0.042	0.045	0.054	0.056	0.084	0.087
1971–1980	−0.015	−0.015	0.592	0.617	0.032	0.040	0.035	0.042	0.074	0.073
1981–1990	−0.034	−0.032	0.641	0.640	0.032	0.035	0.019	0.024	0.041	0.036
1991–2001	−0.052	−0.043	0.665	0.665	0.033	0.038	0.032	0.038	0.013	0.016

Panel B: NBER Definitions of Recession/Expansion Subperiods

Recession	Expansion
Aug 1953–May 1954	Nov 1949–Jul 1953
Sept 1957–Apr 1958	Jun 1954–Aug 1957
May 1960–Feb 1961	May 1958–Apr 1960
Jan 1970–Nov 1970	Mar 1961–Dec 1969
Dec 1973–Mar 1975	Dec 1970–Nov 1973
Feb 1980–July 1980	Apr 1975–Jan 1980
Aug 1981–Nov 1982	Aug 1980–Jul 1981
Aug 1990–Mar 1991	Dec 1982–July 1990
Apr 2001–Dec 2001	Apr 1991–Mar 2001

The National Bureau of Economic Research (NBER) divides the U.S. economy into two economic environments: expansions and recessions. Expansions are from the trough to the peak of business growth and recessions are from the peak to trough of business growth.

Variable Definitions:

NOPACC, or nonoperating accruals = defined as total accruals (net income − cash flow from operations) minus operating accruals (Δaccounts receivable + Δinventories + Δprepaid expenses − Δaccounts payable − Δtaxes payable) plus depreciation and amortization expenses, all divided by beginning period total assets;

SMALL = the percentage of Compustat firms whose total assets are less than 25th percentile of the total assets of firms listed on the New York Stock Exchange for that year;

ΔGDP = the annual percentage change in real gross domestic product, compiled by the U.S. Department of Commerce;

ΔIP = the annual percentage change in total industrial production, compiled by the Federal Reserve; and

CFOA = cash flows from operations divided by beginning period total assets. For the years 1987–2001, we use Compustat item no. 308. From 1971–1986, CFO is funds from operations − Δworking capital (Δcurrent assets + Δ short-term debt − Δcurrent liabilities − Δcash). Prior to 1971, CFO is net income before extraordinary items plus depreciation expense plus Δdeferred income taxes − Δworking capital.

by their total assets and classify as "small" all NYSE, AMEX, and NASDAQ firms that have total assets less than the 25th percentile of NYSE firms.

As Table 2, Panel A shows, both mean and median percentages of all firms on the Compustat tape classified as small rise in the 1960s but explode after 1970 to over 60 percent of the total listings. To a great extent, this pattern parallels Compustat's increased coverage of NASDAQ firms during our time period.[4] For example, in 1950, NASDAQ firms represent 17 percent of the available firms on Compustat (105 out of 615). By 1975 this percentage increases to 55 percent, and to 67 percent by 1997.

In addition, NASDAQ firms are, on average, smaller than exchange-listed firms. For our sample from 1970–2001, the average (median) asset size for NASDAQ firms is $729.1 ($650.2) million, compared to $3,653.6 ($2,594.4) million for NYSE-AMEX firms. In fact, despite the success of many NASDAQ firms in the latter period, the differential in asset size actually grew throughout the time period. In 1951, the average NYSE-AMEX firm was 1.07 times larger than the average NASDAQ firm. By 1970, the differential was 1.79; by 1992, the average NYSE-AMEX firm was 4.35 times larger than the average NASDAQ firm; and by 2000, the multiple reached 6.84. There are various reasons behind this phenomenon. One reason is that the NASDAQ has always had less stringent listing requirements than the NYSE or the AMEX. In particular, minimum listing requirements for revenues and assets consistently have been lower for NASDAQ-traded firms.

The Business Cycle and Macroeconomic Productivity

We represent the U.S. business cycle using three alternative measures of macroeconomic productivity. The first measure is an indicator based on the National Bureau of Economic Research's (NBER) definition of expansionary and recessionary periods. The NBER divides the U.S. economy into two economic environments: expansions are from the trough to the peak of business growth, where business growth is measured in total output, income, and employment (see also Moore 1983), and recessions are measured from the peak to the trough. The NBER uses monthly data such as changes in retail sales, the unemployment rate, real wages, and industrial production (see http://www.nber.org; Hall 1990) to delineate peaks and troughs. As shown in Table 2, Panel B, during 1951–2001, there were nine NBER recession periods and nine NBER expansions. We define the variable *RECESS* as equaling 1 if any part of a recessionary period occurs within a calendar year, and 0 otherwise. Other papers using the NBER definition include Johnson (1999) and Chordia and Shivakumar (2002).

The other two measures are the annual percentage change in real gross domestic product (ΔGDP), and the annual percentage change in total industrial production (ΔIP). GDP is compiled by the U.S. Department of Commerce and includes personal consumption, government expenditures, private investment, inventory growth, and the trade balance (see Bureau of Economic Analysis at http://www.BEA.gov). Papers using ΔGDP as a macroeconomic measure of business productivity include Hall (1990) and Cochrane (1991). Although corporate profits are a component of GDP, we note that from 1979 through 2001, net income (after tax) makes up less than 8 percent of GDP (Federal Reserve Bank of St. Louis 2004) and has fluctuated within the 15–17 percent range after World War II (Coggan 2004). Therefore, examining the relation between accounting losses and ΔGDP does not necessarily impose a mechanistic relation between the two. IP, like GDP, measures total real output, but is more limited in scope. It is compiled by the Federal Reserve and includes total production in manufacturing, mining, gas, and electric utilities (see http://www.

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[4] Unlike the CRSP database, which started covering NASDAQ stocks in 1973, the Compustat database contains NASDAQ-traded stocks throughout our 51-year period. However, the degree of variability in *SMALL* is limited in the first 20 years of our sample period due to relatively few AMEX or NASDAQ firms being listed on Compustat. To overcome this potential problem, we use an alternative size measure, the coefficient of variation of the log of total assets (the standard deviation divided by the mean). We obtain similar results with this measure; the estimated coefficient on this variable is significantly different from zero at conventional levels in each regression.

federalreserve.gov). Previous studies using ΔIP as a measure of business productivity include Fama (1981) and Chordia and Shivakumar (2002). We obtain the data for ΔGDP and ΔIP from "Economagic.com: Economic Time Series." (See http://economagic.com/popular.htm.)

Table 2, Panel A contains the means and medians by decade for ΔGDP and ΔIP. Unlike the data on *NOPACC* and *SMALL*, ΔGDP and ΔIP do not trend upward or downward over time. Instead, we note a higher growth in ΔGDP and ΔIP during the 1960s, followed by a deterioration of economic productivity over the 1970s and 1980s, followed by a subsequent rise in the 1990s.

Cash Flows from Operations

CFOA is CFO divided by beginning total assets averaged across firms for each year. We define CFO according to GAAP. For the years 1987–2001, CFO is annual Compustat item no. 308. From 1971–1986, it is funds from operations (item no. 110) minus the change in working capital (Δcurrent assets + Δdebt in current liabilities − Δcurrent liabilities − Δcash). Prior to 1971, CFO is net income before extraordinary items plus depreciation expense plus the change in deferred taxes minus the change in working capital (see Rayburn 1987).[5]

As shown in the last two columns of Table 2, Panel A, *CFOA* has declined precipitously over time, dropping from a mean (median) of 0.101 (0.104) in the 1950s to 0.013 (0.016) in the 1991–2001 period. Consistent with Joos and Plesko (2005), we also note a drop in *CFOA* from 1971–1990 to 1991–2001. Both our and Joos and Plesko's (2005) findings are in contrast to Givoly and Hayn (2000), who find no marked deterioration in *CFOA* over time. However, Givoly and Hayn (2000) examine a smaller constant-firm sample, which, by construction, asks a different set of questions than those addressed here.[6]

V. EMPIRICAL RESULTS

Empirical Issues

We estimate Equation (1) using *RECESS*, ΔGDP, and ΔIP as alternative measures of *BUSCYC*. Before presenting the results, we discuss several statistical considerations. First, our research design is biased against finding significant coefficients on the business cycle because there is not perfect overlap between the timing of the expansions and contractions and *%LOSSES*. That is, *%LOSSES* is measured on an annual basis, but business cycles begin and end over varying time periods. Thus, our results will be sensitive to possible mismatching between the business cycles and the accounting measurement period.

Second, as we show in Table 3, there is a great deal of collinearity among the independent variables. For example, *CFOA* is highly positively correlated with *NOPACC* (Pearson $\rho = 0.5471$; $p = 0.0001$) and negatively correlated with *SMALL* (Pearson $\rho = -0.8134$; $p = 0.0001$), implying that the drop in *CFOA* has mirrored the rise in non-operating accruals and the percentage of small firms over time. Thus, omitting *CFOA* from the regression would result in upwardly biased coefficients on *NOPACC* and *SMALL* that is due to an omitted correlated variable problem. Similarly, *SMALL* and *NOPACC* are highly negatively correlated (Pearson $\rho = -0.4784$; $p = 0.0004$), suggesting that omitting either variable from the regression on *%LOSSES* would produce an upwardly biased coefficient

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[5] As a sensitivity check, we use the Rayburn (1987) definition of CFO for all years and rerun our regression analyses with this variable. The coefficient and significance level on CFO with the Rayburn measure is qualitatively the same as with CFO defined above.

[6] If we limit our analysis to a constant sample over 1951–2001, we similarly find no significant decrease in *CFOA* over time, consistent with Givoly and Hayn (2000).

TABLE 3
Correlation Coefficients among Frequency of Losses, Accounting Conservatism, Percentage of Small Firms, Macroeconomic Productivity, and CFO

Variable	*%LOSSES*	*NOPACC*	*SMALL*	*RECESS*	ΔGDP	ΔIP	*CFOA*
%LOSSES	1.0000	−0.5567	0.9031	−0.0507	−0.2066	−0.1965	−0.9425
	(0.0000)	(0.0001)	(0.0001)	(0.7237)	(0.1458)	(0.1669)	(0.0001)
NOPACC	−0.5163	1.0000	−0.4784	0.3536	−0.3040	0.0339	0.5471
	(0.0001)	(0.0000)	(0.0004)	(0.0109)	(0.0301)	(0.8134)	(0.0001)
SMALL	0.8741	−0.5850	1.0000	−0.0854	−0.1829	−0.1693	−0.8134
	(0.0001)	(0.0001)	(0.0000)	(0.5511)	(0.1990)	(0.2351)	(0.0001)
RECESS	−0.0205	0.4035	−0.1637	1.0000	−0.7422	−0.6092	0.2141
	(0.8867)	(0.0033)	(0.2510)	(0.0000)	(0.0001)	(0.0001)	(0.1314)
ΔGDP	−0.2512	−0.2744	−0.1345	−0.7133	1.0000	0.5421	0.1039
	(0.0754)	(0.0514)	(0.3468)	(0.0001)	(0.0000)	(0.0001)	(0.4679)
ΔIP	−0.2268	0.0070	−0.1555	−0.5760	0.5602	1.0000	0.0667
	(0.1096)	(0.9609)	(0.2760)	(0.0001)	(0.0001)	(0.0000)	(0.6418)
CFOA	−0.9203	0.5752	−0.8622	0.2280	0.1386	0.0985	1.0000
	(0.0001)	(0.0001)	(0.0001)	(0.1075)	(0.3321)	(0.4916)	(0.0000)

Pearson (Spearman) correlation coefficients are presented above (below) the diagonal.
p-values are presented in parentheses.

Variable Definitions:

%LOSSES = the percentage of accounting losses reported for each year;

NOPACC = nonoperating accruals defined as total accruals (net income − cash flow from operations) minus operating accruals (Δaccounts receivable + Δinventories + Δprepaid expenses − Δaccounts payable − Δtaxes payable) plus depreciation and amortization expenses, all divided by beginning period total assets;

SMALL = the percentage of Compustat firms whose total assets are less than 25th percentile of the total assets of firms listed on the New York Stock Exchange for that year;

RECESS = 1 if the year is included in an NBER recession period, and 0 otherwise. The NBER divides the U.S. economy into two economic environments: expansions and recessions. Expansions are from the trough to the peak of business growth and recessions are from the peak to trough of business growth;

ΔGDP = the annual percentage change in real gross domestic product. Real GDP is compiled monthly by the U.S. Department of Commerce;

ΔIP = the annual percentage change in total industrial production. It is compiled monthly by the Federal Reserve; and

CFOA = cash flows from operations divided by beginning period total assets. For the years 1987–2001, we use Compustat item no. 308. From 1971–1986, CFO is funds from operations minus Δworking capital (Δcurrent assets + Δ short-term debt − Δcurrent liabilities − Δcash). Prior to 1971, CFO is net income before extraordinary items plus depreciation expense plus Δdeferred income taxes − Δworking capital.

on the remaining variable. These correlations have implications both for our study and for interpretations of other studies, as they imply that examining bivariate associations between accounting losses (accounting earnings) and a single variable in isolation may result in biased inferences.

Third, *CFOA* is uncorrelated with the business cycle variables. The Pearson correlation coefficient between *CFOA* and *RECESS* is 0.2141 ($p = 0.1314$); between *CFOA* and ΔGDP it is 0.1039 ($p = 0.4679$); and between *CFOA* and ΔIP it is 0.0667 ($p = 0.6418$). These correlations buttress our assertion that *CFOA* primarily measures firm-specific performance as opposed to being a reflection of the business cycle.

Fourth, although *CFOA* is uncorrelated with the business cycle variables, it is related significantly to *%LOSSES* (Pearson $\rho = -0.9428$; $p = 0.0001$). Therefore, omitting *CFOA* from Equation (1) would lead to an upwardly biased estimator of the variance of β_3, leading to an acceptance of the null hypothesis more frequently than is justified by the given level of significance (Kmenta 1971).

Regression Results

The regressions results are presented in Table 4. We begin by examining the simple regression of *%LOSSES* on accounting conservatism (*NOPACC*) alone. Consistent with Givoly and Hayn (2000), we find a negative relation between the percentage of accounting losses over time and *NOPACC*. The regression coefficient on *NOPACC* is −3.359, with a t-statistic of −5.62 (p-value < 0.01). Because more negative levels of *NOPACC* indicate greater accounting conservatism, these findings are consistent with the view that the annual

TABLE 4

OLS Regressions of Frequency of Losses on Accounting Conservatism, Percentage of Small Firms, Macroeconomic Productivity, and CFO

$$\%LOSSES_t = \beta_0 + \beta_1 NOPACC_t + \beta_2 SMALL_t + \beta_3 BUSCYC_t + \beta_4 CFOA_t + \varepsilon_t$$

Model	Intercept	*NOPACC*	*SMALL*	*BUSCYC*	*CFOA*	Adj. R^2
1	0.095***	−3.359***				38.0%
	(4.36)	(−5.62)				
2	−0.180***	−1.252***	0.796***			85.2%
	(−7.40)	(−3.72)	(12.55)			
3	0.150***	−0.491***	0.374***	0.041***	−2.408***	95.6%
β_3 = *RECESS*	(4.22)	(−3.36)	(7.04)	(4.43)	(−10.36)	
4	0.167***	−0.621**	0.376***	−0.001***	−2.181***	94.9%
β_3 = *ΔGDP*	(4.29)	(−2.61)	(6.55)	(−3.29)	(−8.82)	
5	0.168***	−0.330	0.378***	−0.002***	−2.328***	94.6%
β_3 = *ΔIP*	(4.14)	(−1.44)	(6.36)	(−2.74)	(−9.09)	

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test.

In Panel B, t-statistics are presented in parentheses.

Variable Definitions:

%LOSSES = the percentage of accounting losses reported for each year;

NOPACC = mean nonoperating accruals, defined as total accruals (net income − cash flow from operations) minus operating accruals (Δaccounts receivable + Δinventories + Δprepaid expenses − Δaccounts payable − Δtaxes payable) plus depreciation and amortization expenses, all divided by beginning period total assets;

SMALL = the percentage of Compustat firms whose total assets are less than 25th percentile of the total assets of firms listed on the New York Stock Exchange for that year;

BUSCYC = the U.S. business cycle, which we represent using three different measures. The National Bureau of Economic Research (NBER) divides the U.S. economy into two economic environments: expansions and recessions. Expansions are from the trough to the peak of business growth and recessions are from the peak to trough of business growth;

RECESS = 1 if the year is included in an NBER recession period, and 0 otherwise;

ΔGDP = the annual percentage change in real gross domestic product. Real GDP is compiled monthly by the U.S. Department of Commerce;

ΔIP = the annual percentage change in total industrial production. It is compiled monthly by the Federal Reserve; and

CFOA = cash flows from operations divided by beginning period total assets. For the years 1987–2001, we use Compustat item no. 308. From 1971–1986, CFO is funds from operations minus Δworking capital (Δcurrent assets + Δ short-term debt − Δcurrent liabilities − Δcash). Prior to 1971, CFO is net income before extraordinary items plus depreciation expense plus Δdeferred income taxes − Δworking capital.

percent of accounting losses is positively related to the degree of accounting conservatism. The adjusted R^2 indicates that 38.0 percent of the variation in the percentage of annual losses can be explained by accounting conservatism alone. Thus, we corroborate Givoly and Hayn's (2000) results for a larger sample of firms.

In model 2, *SMALL* is added to the regression. The estimated coefficient on *SMALL* is 0.796, with a t-statistic of 12.55 (p-value < 0.01). Thus, as predicted, the temporal increase in the percentage of accounting losses is mirrored by the temporal increase in *SMALL*. The adjusted R^2 is 85.2 percent, compared to an adjusted R^2 of 38.0 percent for the simple regression of *%LOSSES* on *NOPACC*. Testing for the incremental explanatory power of adding *SMALL* yields an F-statistic of 67.1, significant at the 1 percent level, supporting the hypothesis that *%LOSSES* depends not only on *NOPACC*, but also on *SMALL*.

The drop in the estimated coefficient on *NOPACC* from −3.359 in model 1 to −1.252 in model 2 reflects the highly negative correlation between *NOPACC* and *SMALL* that we documented in Table 3.[7] Nonetheless, both variables provide significant explanatory power over the frequency of accounting losses even in the presence of this multicollinearity.

In models 3 through 5, we add the business cycle (*RECESS*, ΔGDP, or ΔIP) and cash flows from operations (*CFOA*) to the multivariate regression. As these regressions show, there are significant associations between *%LOSSES* and the business cycle variables. The coefficients on ΔGDP and ΔIP are negative at the 0.01 level of significance, indicating that *%LOSSES* is negatively related to macroeconomic growth. The coefficient on *RECESS* is significantly positive at the 1 percent level; i.e., *%LOSSES* are higher for years in which there is a recession.

The coefficients on *CFOA* are also significantly negative, consistent with the view that accounting losses are negatively related to the company's firm-specific economic productivity. For both regressions, the estimated coefficient on *CFOA* is statistically significant at the 0.01 level. The regression coefficients on *SMALL* (*NOPACC*) remain significantly positive (negative), with the exception of model 5, in which *NOPACC* is negative but insignificantly different from zero.[8]

To examine the effects of adding these variables to the model, we compare the coefficients on *NOPACC* and *SMALL* from models 3–5 to the regression on *NOPACC* and *SMALL* alone (model 2). The magnitude of the estimated coefficient on *NOPACC* drops from −1.252 to −0.491 in model 3, −0.621 in model 4, and −0.330 in model 5. The coefficient on *SMALL* drops from 0.796 in model 2 to around 0.375 in models 3–5. The explanation behind these declines lies in the significant negative correlations between *CFOA* and *NOPACC* and between *CFOA* and *SMALL*. That is, adding *CFOA* to the regression model reduces the biases in these coefficients.

In summary, we find that accounting losses are related to both accounting conservatism and nonaccounting factors. Specifically, the percentage of losses is directly related to accounting conservatism and the percentage of small firms on Compustat's database; it is inversely related to the business cycle and to cash flows from operations. Thus, nonaccounting fundamentals add significant incremental information about accounting losses over and beyond accounting conservatism.

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[7] The significant correlation between *NOPACC* and *SMALL* raises the question of whether *SMALL* is truly a "nonaccounting" factor, as we claim. Although the two variables are highly correlated, a regression of *NOPACC* on *SMALL* and a time trend variable reveals that only the trend variable is significant in explaining *NOPACC* levels over time, consistent with our characterization of *SMALL* as unrelated to accounting conservatism.

[8] As an alternative measure of cash flows, we use free cash flows instead of cash flows from operations. We define free cash flows as our CFO measure minus capital expenditures (annual Compustat item #128). The correlation between CFO and free cash flows is 0.94. When we substitute free cash flows for CFO in models (3) through (5), we find that the results are qualitatively the same, but weaker. For example, in model (4), the adjusted R^2 falls to 87.8 percent and the intercept becomes insignificantly different from zero. We thus conclude that CFO is a better indicator of real firm performance than free cash flows.

VI. ROBUSTNESS TESTS

Using *ROA* as the Dependent Variable

In this section, we examine whether our results apply only to losses or whether they can be generalized to net income-to-total assets (hereafter *ROA*). Givoly and Hayn (2000) define profitability as *ROA* and show a relation between the deterioration of *ROA* over time and the rise of accounting conservatism. Since this paper builds on their work, we consider whether our findings hold for *ROA* over time as well as losses. Further, by understanding the nonaccounting fundamentals behind *ROA*, academics and other users of accounting income can better utilize and interpret net income in general.

We estimate the following regression:

$$ROA_t = \beta_0 + \beta_1 NOPACC_t + \beta_2 SMALL_t + \beta_3 BUSCYC_t + \beta_4 CFOA_t + \varepsilon_t \tag{2}$$

where ROA_t is the median ROA for year *t*. We measure *ROA* as net income (annual Compustat item no. 172) divided by beginning period total assets.

Table 5, Panel A presents the mean and median *ROA* by decade. Consistent with Givoly and Hayn (2000), there is a steady deterioration of *ROA* over time. In the 1950s, the median *ROA* is 0.068. By the 1980s, the median *ROA* is less than half that amount (0.031), and it falls by almost 50 percent again in the 1990s (0.016). The mean *ROA* shows similar declines, turning negative in the 1980s and remaining negative throughout the 1990s.

Table 5, Panel B shows the summary statistics for Equation (2) using the three definitions of macroeconomic productivity. The results complement those presented in Table 4. The estimated coefficients on *SMALL*, *ΔGDP*, *RECESS*, and *CFOA* are significantly different from zero at the 0.01 levels and are in the predicted directions. *ΔIP* is significantly positive at the 0.10 level, and *NOPACC*, the accounting conservatism variable, is insignificantly different from zero. The adjusted R^2 values for the regressions with *ROA* are comparable to those reported with *%LOSSES* as the dependent variable. We conclude that the analyses presented thus far are fairly robust to whether we examine determinants behind accounting losses or income levels, in general. In particular, we find that *nonaccounting* factors influence losses and *ROA* in similar ways.

Alternative Measures of Accounting Conservatism

Several studies assess alternative measures of accounting conservatism and conclude these measures reflect different aspects of the accounting process (Watts 2003; Givoly et al. 2004; Roychowdhury and Watts 2004). To examine the extent to which differing accounting conservatism measures affect our regression analyses, we use two commonly used alternative measures as substitutes for *NOPACC*.

The first measure is Basu's (1997) earnings-returns (*E-R*) metric, which is the γ_2 coefficient in the following regression:

$$EPS_{it}/Price_{i,t-1} = \alpha_0 + \alpha_1 DUM_{it} + \gamma_1 RETURN_{it} + \gamma_2(RETURN_{it}*DUM_{it}) + \varepsilon_{it} \tag{3}$$

where the *i* and *t* subscripts denote the firm and year, respectively; *EPS* is annual earnings

TABLE 5
Using Return on Assets (*ROA*) as the Dependent Variable

Panel A: *ROA* by Decade

Time Period	Mean	Median
1951–1960	0.072	0.068
1961–1970	0.056	0.054
1971–1980	0.042	0.047
1981–1990	−0.023	0.031
1991–2001	−0.096	0.016

Panel B: OLS Regressions of *ROA* on Accounting Conservatism, Percentage of Small Firms, Macroeconomic Productivity, and CFO

$$ROA_t = \beta_0 + \beta_1 NOPACC_t + \beta_2 SMALL_t + \beta_3 BUSCYC_t + \beta_4 CFOA_t + \varepsilon_t$$

Model	Intercept	*NOPACC*	*SMALL*	*BUSCYC*	*CFOA*	Adj. R^2
1	0.027***	−0.010	−0.027***	0.001***	0.405***	94.8%
$\beta_3 = \Delta GDP$	(4.39)	(−0.26)	(−2.99)	(3.59)	(11.54)	
2	0.024***	0.010	−0.027***	−0.001***	0.430***	92.8%
$\beta_3 = RECESS$	(3.73)	(0.26)	(−2.86)	(−2.86)	(10.41)	
3	0.025***	−0.032	−0.029***	0.0003*	0.449***	92.2%
$\beta_3 = \Delta IP$	(3.64)	(−0.82)	(−2.85)	(1.85)	(10.32)	

*, **, and *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test.

In Panel B, t-statistics are presented in parentheses.

Variable Definitions:

ROA = the median return on assets for all firms in year *t*;

NOPACC, nonoperating accruals = defined as total accruals (net income − cash flow from operations) minus operating accruals (Δaccounts receivable + Δinventories + Δprepaid expenses − Δaccounts payable − Δtaxes payable) plus depreciation and amortization expenses, all divided by beginning period total assets;

SMALL = the percentage of Compustat firms whose total assets are less than 25th percentile of the total assets of firms listed on the New York Stock Exchange for that year;

BUSCYC = the U.S. business cycle, which we represent using three different measures. The National Bureau of Economic Research (NBER) divides the U.S. economy into two economic environments: expansions and recessions. Expansions are from the trough to the peak of business growth and recessions are from the peak to trough of business growth;

RECESS = 1 if the year is included in an NBER recession period, and 0 otherwise;

Δ*GDP* = the annual percentage change in real gross domestic product. Real GDP is compiled monthly by the U.S. Department of Commerce;

Δ*IP* = the annual percentage change in total industrial production. It is compiled monthly by the Federal Reserve; and

CFOA = cash flows from operations divided by beginning period total assets. For the years 1987–2001, we use Compustat item no. 308. From 1971–1986, CFO is funds from operations minus Δworking capital (Δcurrent assets + Δ short-term debt − Δcurrent liabilities − Δcash). Prior to 1971, CFO is net income before extraordinary items plus depreciation expense plus Δdeferred income taxes − Δworking capital.

per share; *Price* is the stock price at the beginning of the fiscal year; *DUM* is a dummy variable equal to 1 if the stock return is negative, and 0 otherwise; and *RETURN* is the year's stock return. Individual stock returns are computed over the 12-month period beginning nine months prior to the end of fiscal year *t*. Under Basu's (1997) methodology, $\gamma_2(E\text{-}R)$ is expected to be positive. The underlying notion behind this premise is that, with conservative accounting, earnings reflect bad news more quickly than they reflect good news. Thus, we predict a positive relation between *%LOSSES* and *E-R*.

The second measure is the annual market-to-book ratio of equity (see Beaver and Ryan 2000). This measure of conservatism recognizes that the market's valuation of a firm's assets and liabilities can differ substantially from the book value of these assets and liabilities. A ratio greater than 1.0 indicates accounting conservatism of the balance sheet. Accounting reasons for the discrepancies between market and book value of equity include nonrecognition of intangibles (e.g., R&D, brand names, and market shares), using historical costs instead of market values for property, plant, and equipment, and off-balance sheet financings and contingencies. We predict a positive relation between *%LOSSES* and the market-to-book ratio (*MB*).

Table 6, Panel A shows the mean and median values for *E-R* and *MB* by decade. Consistent with prior research (e.g., Basu 1997), mean and median *E-R* increase over time, peaking during the 1980s, but still remaining relatively high in the 1990s. In contrast, *MB* increases between the 1950s and 1960s, dips dramatically in the 1960s, then rises precipitously over the next two decades. Comparing *E-R* and *MB* to *NOPACC* and to each other suggests that the individual accounting conservatism measures are picking up different aspects of financial reporting.[9]

Panel B of Table 6 contains the regression results; models 1–3 present the estimated coefficients on *E-R*, and models 4–6 show the coefficients on *MB*. As Panel B shows, the nonaccounting variables, *SMALL*, *BUSCYC*, and *CFOA*, are robust to each accounting conservatism metric. In all six regressions, the coefficients on the nonaccounting variables are statistically significant at conventional levels, with all but one variable significant at the 0.05 or 0.01 levels.

In contrast, we find very different statistical results for the accounting conservatism variables. In models 1–3, *E-R* is statistically positive at the 0.05 or 0.01 levels, indicating a significant association between *%LOSSES* and the *E-R* conservatism measure. In models 4–6, the estimated coefficients on *MB* are insignificantly different from zero, suggesting no relation between *MB* and accounting losses. Further, when we compare the coefficients in Table 6 to those in Table 4 (when we used *NOPACC* as our measure of accounting conservatism), we see that using *E-R* has no tangible effects on the estimated coefficients on *BUSCYC* and *CFOA*, but diminishes the coefficient on *SMALL*. Conversely, using *MB* has no discernible impact on the coefficients on *BUSCYC* and *SMALL*, but reduces the coefficients on *CFOA*. We find that the primary causes of these differences are reflected in the correlations between the independent variables. *E-R* and *SMALL* have a 0.63 correlation statistic, and *MB* and *CFOA* have a correlation coefficient of −0.42. Thus, consistent with Givoly et al. (2004) and Roychowdhury and Watts (2004), we find differences in accounting conservatism metrics. However, we also find that these differences contribute little to our interpretation of how nonaccounting factors relate to accounting losses.

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[9] The Pearson correlation coefficient between *NOPACC* and *MB* is −0.18 ($p = 0.20$); the coefficient between *NOPACC* and *E-R* is −0.21 ($p = 0.14$); and the coefficient between *MB* and *E-R* is 0.03 ($p = 0.84$).

TABLE 6
Sensitivity Analysis for Alternative Accounting Conservatism Measures

Panel A: Alternative Conservatism Measures by Decade

	Earnings-Return Measure (*E-R*)		Market-to-Book Ratio (*MB*)	
Time Period	**Mean**	**Median**	**Mean**	**Median**
1951–1960	0.093	0.122	2.02	2.03
1961–1970	0.110	0.105	2.61	2.39
1971–1980	0.296	0.318	1.64	1.30
1981–1990	0.408	0.422	2.49	2.80
1991–2000	0.365	0.309	3.02	3.39

Panel B: OLS Regression of Frequency of Losses on Alternative Accounting Conservatism Measures, Percentage of Small Firms, Macroeconomic Productivity, and CFO

$$\%LOSSES_t = \beta_0 + \beta_1 ALTCNSV_t + \beta_2 SMALL_t + \beta_3 BUSCYC_t + \beta_4 CFOA_t + \varepsilon_t$$

Model	**Intercept**	***E-R***	***SMALL***	***BUSCYC***	***CFOA***	**Adj. R²**
1	0.277***	0.130***	0.125*	−0.005**	−2.883***	93.6%
$\beta_3 = \Delta GDP$	(5.66)	(3.88)	(1.97)	(−2.44)	(−11.54)	
2	0.210***	0.082**	0.280***	−0.002***	−2.565***	94.1%
$\beta_3 = \Delta IP$	(5.71)	(2.59)	(4.21)	(−2.63)	(11.79)	
3	0.251***	0.111***	0.145**	0.035***	−3.003***	94.1%
$\beta_3 = RECESS$	(5.94)	(3.36)	(2.37)	(3.22)	(−12.72)	
	Intercept	***MB***	***SMALL***	***BUSCYC***	***CFOA***	**Adj. R²**
4	0.236***	−0.007	0.382***	−0.005**	−3.327***	91.9%
$\beta_3 = \Delta GDP$	(3.55)	(−0.75)	(4.34)	(−2.17)	(−13.42)	
5	0.244***	−0.007	0.356***	−0.002**	−3.377***	91.9%
$\beta_3 = \Delta IP$	(3.69)	(−0.82)	(4.04)	(−2.28)	(−13.84)	
6	0.206***	−0.003	0.381***	0.040***	−3.457***	92.7%
$\beta_3 = RECESS$	(3.26)	(−0.37)	(4.58)	(3.23)	(−14.92)	

*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test.

In Panel B, t-statistics are presented in parentheses.

Variable Definitions:

%LOSSES = the percentage of accounting losses reported for each year;

E-R = the estimated coefficient γ_2 from the following annual cross-sectional regression:

$$EPS_{it}/Price_{i-1} = \alpha_0 + \alpha_1 DUM_{it} + \gamma_1 Return_{it} + \gamma_2(Return_{it}*DUM_{it}) + \varepsilon_{it},$$

where EPS_{it} is the earnings per share of firm *i* in fiscal year *t*, $Price_{i,t-1}$ is the price per share at the beginning of the fiscal year, $Return_{it}$ is the return of firm *i* over the 12 months beginning nine months prior to the end of the fiscal year *t*, and DUM_{it} is a dummy variable set equal to 1 if R_{it} is negative, and 0 otherwise;

MB = market price per share divided by book value per share at the end of the fiscal year;

SMALL = the percentage of Compustat firms whose total assets are less than 25th percentile of the total assets of firms listed on the New York Stock Exchange for that year;

BUSCYC = the U.S. business cycle, which we represent using three different measures. The National Bureau of Economic Research (NBER) divides the U.S. economy into two economic environments: expansions and recessions. Expansions are from the trough to the peak of business growth and recessions are from the peak to trough of business growth;

(*continued on next page*)

TABLE 6 (Continued)

RECESS = 1 if the year is included in an NBER recession period, and 0 otherwise;
ΔGDP = the annual percentage change in real gross domestic product. Real GDP is compiled monthly by the U.S. Department of Commerce;
ΔIP = the annual percentage change in total industrial production. It is compiled monthly by the Federal Reserve; and
CFOA = cash flows from operations divided by beginning period total assets. For the years 1987–2001, we use Compustat item no. 308. From 1971–1986, CFO is funds from operations minus Δworking capital (Δcurrent assets + Δ short-term debt − Δcurrent liabilities − Δcash). Prior to 1971, CFO is net income before extraordinary items plus depreciation expense plus Δdeferred income taxes − Δworking capital.

How Important are Time-Series Trends?

The results reported thus far are based on regressions in which one level variable is regressed on several other level variables. As Table 4 shows, both the t-statistics on the coefficients as well as the adjusted R^2 value for the full regression are high. However, two of the independent variables, *SMALL* and *CFOA*, display either upward or downward trends, as does the dependent variable, *%LOSSES*. These trends raise several questions. First, are the high adjusted R^2 values reported in Table 4 related to these trends? Second, and perhaps more importantly, to what extent do these trends contribute to the documented rise in accounting losses over time? Finally, after controlling for the trends, what role does the business cycle play in explaining accounting losses?

We begin answering these questions by noting that the Durbin-Watson statistics for the regressions reported in Table 4 are significant at the 0.01 level, suggesting some degree of autocorrelation among the residuals. To control for the autocorrelation in the disturbances in the regression models and to remove possible trends in the data, we perform two separate tests. First, we transform the data into first differences and rerun the regression analyses on the transformed data. The regression model is:

$$\Delta\%LOSSES_t = \beta_0 + \beta_1\Delta NOPACC_t + \beta_2\Delta SMALL_t + \beta_3 BUSCYC_t + \beta_4\Delta CFOA_t + \varepsilon_t \quad (4)$$

where Δ is the one-year change. The subscript t refers to the change between years $t-1$ and t. Note that all variables except the business cycle variables (ΔGDP, ΔIP, and *RECESS*) have been differenced to take out their trend factors. We do not difference our business cycle variables, as they are already expressed as a change from the prior year.

Second, we incorporate a time trend factor, *TREND*, into the original levels regression, where *TREND* is defined as the calendar year minus 1950. The regression model is as follows:

$$\%LOSSES_t = \beta_0 + \beta_1 NOPACC_t + \beta_2 SMALL_t + \beta_3 BUSCYC_t + \beta_4 CFOA_t + \beta_5 TREND_t + \varepsilon_t. \quad (5)$$

The difference between Equations (4) and (5) reflects dissimilar statistical artifacts. Equation (4) corrects for the presence of a first-degree autocorrelation process in the dependent and independent variables. Including the trend factor in Equation (5) is a correct procedure for eliminating the presence of a time-stationary process.

Table 7 presents the summary statistics for each regression. In Panel A, *Δ%LOSSES* is significantly related to ΔGDP, ΔIP, and *RECESS* at the 0.01 level. Thus, the yearly change

TABLE 7
Examination of Time-Series Trends in the Data

Panel A: OLS Regression of Change in Percentage of Accounting Losses on Changes in Accounting Conservatism, Percentage of Small Firms, Macroeconomic Productivity, and CFO

$$\Delta\%LOSSES_t = \beta_0 + \beta_1\Delta NOPACC_t + \beta_2\Delta SMALL_t + \beta_3 BUSCYC_t + \beta_4\Delta CFOA_t + \varepsilon_t$$

Model	Intercept	$\Delta NOPACC$	$\Delta SMALL$	$BUSCYC$	$\Delta CFOA$	Adj. R^2
1	0.027***	0.035	0.370**	−0.006***	−0.316	32.1%
$\beta_3 = \Delta GDP$	(4.33)	(0.19)	(2.01)	(−4.01)	(−0.93)	
2	0.019***	−0.107	0.249*	−0.004***	−1.10***	57.1%
$\beta_3 = \Delta IP$	(5.79)	(−0.72)	(1.68)	(−7.19)	(−3.73)	
3	−0.003	0.035	0.410**	0.029***	−0.671*	30.0%
$\beta_3 = RECESS$	(−0.78)	(0.19)	(2.21)	(3.78)	(−1.86)	

Panel B: OLS Regression of Percentage of Accounting Losses Accounting Conservatism, Percentage of Small Firms, Macroeconomic Productivity, CFO, and a Trend Factor

$$\%LOSSES_t = \beta_0 + \beta_1 NOPACC_t + \beta_2 SMALL_t + \beta_3 BUSCYC_t + \beta_4 CFOA_t + \beta_5 TREND_t + \varepsilon_t$$

Model	Intercept	$NOPACC$	$SMALL$	$BUSCYC$	$CFOA$	$TREND$	Adj. R^2
1	0.142***	−0.575**	0.286***	−0.007***	−1.842***	0.002	94.9%
$\beta_3 = \Delta GDP$	(3.21)	(−2.40)	(3.02)	(−3.24)	(−4.88)	(1.18)	
2	0.140***	−0.285	0.278***	−0.002***	−1.953***	0.002	94.7%
$\beta_3 = \Delta IP$	(3.07)	(−1.24)	(2.85)	(−2.75)	(−5.02)	(1.27)	
3	0.125***	−0.449**	0.284***	0.041***	−2.065***	0.002	96.7%
$\beta_3 = RECESS$	(3.10)	(−2.13)	(3.23)	(4.41)	(−5.85)	(1.28)	

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*, **, *** Indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively, using a two-tailed test.

In Panel A, Δ indicates the change as the value in year t minus the value in year $t-1$.

In Panel B, t-statistics are presented in parentheses.

Variable Definitions:

%LOSSES = the percentage of accounting losses reported for each year;

NOPACC, nonoperating accruals = defined as total accruals (net income − cash flow from operations) minus operating accruals (Δaccounts receivable + Δinventories + Δprepaid expenses − Δaccounts payable − Δtaxes payable) plus depreciation and amortization expenses, all divided by beginning period total assets;

SMALL = the percentage of Compustat firms whose total assets are less than 25th percentile of the total assets of firms listed on the New York Stock Exchange for that year;

BUSCYC = the U.S. business cycle, which we represent using three different measures. The National Bureau of Economic Research (NBER) divides the U.S. economy into two economic environments: expansions and recessions. Expansions are from the trough to the peak of business growth and recessions are from the peak to trough of business growth;

RECESS = 1 if the year is included in an NBER recession period, and 0 otherwise;

ΔGDP = the annual percentage change in real gross domestic product. Real GDP is compiled monthly by the U.S. Department of Commerce;

ΔIP = the annual percentage change in total industrial production. It is compiled monthly by the Federal Reserve;

(continued on next page)

TABLE 7 (Continued)

CFOA = cash flows from operations divided by beginning period total assets. For the years 1987–2001, we use Compustat item no. 308. From 1971–1986, CFO is funds from operations minus Δworking capital (Δcurrent assets + Δ short-term debt − Δcurrent liabilities − Δcash). Prior to 1971, CFO is net income before extraordinary items plus depreciation expense plus Δdeferred income taxes − Δworking capital; and
TREND = the calendar year minus 1950.

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in the percentage of losses is significantly related to the business cycle, suggesting that changes in the frequency of accounting losses are exaggerated or attenuated by the business cycle. *Δ%LOSSES* is also positively and significantly associated with *ΔSMALL*, the change in the percentage of small firms in the Compustat database. Thus, as relatively more small firms are added (or removed) each year from Compustat's coverage, the percentage of accounting losses increases or decreases as well. For the regressions with *ΔIP* and *RECESS*, *Δ%LOSSES* is significantly negatively related to the annual change in *CFOA*, supporting our earlier finding that accounting losses are inversely related to cash flows from operations. The main difference between the levels-based and changes-based regressions, however, is in the significance levels for *NOPACC vis-à-vis ΔNOPACC*. Whereas in Table 4, *NOPACC* is significantly negative for two of the three levels-based models, none of the coefficients on *ΔNOPACC* is significantly different from zero in the changes-based models. Thus, over time, there appears to be no systematic relation between changes in the frequency of accounting losses and changes in the degree of accounting conservatism, *ceteris paribus*.

The adjusted R^2 values for the models are 32.1 percent, 57.1 percent, and 30.0 percent, respectively. Although the explanatory power for the changes regressions are lower than that for the levels regressions, they still suggest that a significant amount of variation in the change in the percentage of losses is picked up by changes in nonaccounting variables. The Durbin-Watson statistics for each regression are insignificantly different from zero, implying that the autocorrelation factors have been removed. The insignificant coefficients on *ΔNOPACC* and the reduced significance levels on *ΔSMALL* and *ΔCFOA* suggest that much of the time-series correction is coming from these three factors.

Table 7, Panel B contains the regression results for the levels-based equation with a time-trend factor included as an additional independent variable. The estimated coefficient on the trend factor is insignificantly different from zero (t-statistics range from 1.18 to 1.28) for the three regressions. In addition, the interpretation of the independent variables with respect to their relation to *%LOSSES* is unaffected by the inclusion of a time-trend factor. However, consistent with the changes-based regressions, the t-statistics on *SMALL* and *CFOA* drop dramatically from those reported in Table 4. For example, for the regression using *ΔGDP* to measure macroeconomic productivity, the t-statistic for *SMALL* drops from 6.55 in Table 4 to 3.02 in Table 7. Similarly, the t-statistic for *CFOA* declines from −8.82 to −4.88. In contrast, there is little to no change in the t-statistics for *NOPACC* or for the business cycle variables. Thus, as with the changes-based analysis, adding the trend factor corrects for the time-series properties of the *SMALL* and *CFOA* variables.

VII. WHY DO SMALL FIRMS REPORT MORE LOSSES?

Our results are consistent with the view that that the increasing percentage of small firms on Compustat is a major reason for the increase in the percentage of firms reporting losses over time. In our hypothesis section, we posited several reasons behind this phenomenon: small firms are less diversified, they are more risky, and they are more likely to be in the early or later stages of their life-cycle. In this section, we investigate the associations among small firms, losses, and these variables.

First, we posit that small firms are less diversified. If small firms are less diversified, then they are limited in their abilities to offset losses from one line of business against gains from another line of business. One direct way to measure this is to compare the number of reporting segments between small and other firms. Firms with only one segment are the least able to offset losses. We therefore predict that small firms are more likely to be single-segmented. We also predict that the likelihood of reporting a loss is higher for firms with a single business segment.

Second, we propose that small firms have more volatility in their operations and, therefore, are more likely to report losses in one year and profits in another year. In contrast, larger firms have more smoothed performances in their operations and consequently are less likely to report losses in any given year. One way to measure operations volatility is to examine the idiosyncratic component of the firm's variance of daily stock returns over the year. Campbell et al. (2001) specifically link idiosyncratic stock volatility to the variance of cash flow shocks. We predict that small firms have higher idiosyncratic risk than larger firms and, consequently, the likelihood of a firm reporting a loss is positively related to its idiosyncratic risk over time.

Third, we posit that small firms are more likely to be in the beginning or ending stages of their life-cycles. Fama and French (2004) show that from 1980–1989, 89.7 percent of new common stock listings (IPOs and non-IPOs) were in the 0 to 20th percentile of market equity, based on all stocks trading on the NYSE in the month of the listing. From 1990–2000, 72.0 percent of new listings were in the 0 to 20th percentile of market equity. Fama and French (2004) also demonstrate that the percentage of new listings reporting accounting losses in their first year was 44.2 percent in 1980–1989 and 44.2 percent in 1990–2000 for IPOs, and was 44.6 percent in 1989–1989 and 51.6 percent in 1990–2000 for non-IPO new listings. Taken together, their results suggest that many small firms are in the beginning of their life-cycle and that newly listed firms have a high probability of showing an accounting loss in its listing year. We therefore propose that firm size is positively related to it being a new listing and that the likelihood of a firm reporting an accounting loss is positively related to whether it is a new listing. We also hypothesize that small firms in the earlier stages of development invest more heavily in long-term assets and research and development than larger firms and so are less likely to be reporting positive cash flows from operations and/or positive net income. We therefore posit that small firms will have more negative cash flows from investing (CFI) (with R&D included) than larger firms and that the likelihood of a firm reporting an accounting loss is negatively related to its investing cash flows.

Barth et al. (1998) show that firms delisting from Compustat because of bankruptcy between 1975–1993 have, in the year prior to delisting, mean assets of \$133.85 million and mean net income of −\$67.93 million. They also demonstrate that over 1988–1993, financially healthier firms have substantially higher market values of equity and net income than firms with lower financial health. Their findings suggest that many small firms are in the end of their life-cycle and that these firms are more likely to report losses in their delisting year. Consequently, we posit that small firms are more frequently delisted and that accounting losses are related to whether a firm delists.

Data on the number of business segments are from Compustat's segment detail file. *SEG1* is a dummy variable equal to 1 if the firm has only one business segment, and 0 otherwise. Segment data are available from 1985 onward. Idiosyncratic risk (*IDVOL*) is the standard deviation of the residual from a firm-specific regression of daily returns on the equally weighted CRSP market portfolio returns, estimated annually. *NEW* is a dummy variable equal to 1 if the firm appears for the first time in year *t* on the Compustat tape, and 0 otherwise. *DELIST* is a dummy variable equal to 1 if the firm's history on the Compustat research tape ends at year *t*, and 0 otherwise. *CFI* is cash flows from investing (annual Compustat item no. 311) minus research and development expense (annual Compustat item no. 46), divided by total assets. These data are available from 1987 onward.

We perform both univariate and multivariate analyses. Because our data on CFI begin in 1987, we limit our analysis to firms listed on Compustat in 1987–2001. Since this gives us only 15 years of data, we do not use annually aggregated data; instead, we use firm-specific data. Thus, *SMALLFIRM* is a dummy variable equal to 1 if the firm is at or below the 25th percentile of NYSE assets for that year, and 0 otherwise. *LOSSFIRM* is a dummy variable equal to 1 is the firm has negative net income in that year, and 0 otherwise.

Table 8, Panel A presents the univariate tests on differences in firm characteristics for small/large firms and for loss/profit firms. First, we show that small firms are significantly more likely to report losses than large firms (46.58 percent versus 18.58 percent), or, alternatively, loss firms are significantly more likely to be small in size than profit firms (74.05 percent versus 42.75 percent). In addition, the Chi-square statistic from a 2 × 2 table of size versus profitability is 6,257.8, which is highly significant (the cut-off for a Chi-square with one degree of freedom is 15.13 at the 0.0001 level of significance). Thus, the relationship between small firm size and the tendency to report losses is unequivocal.

As Panel A of Table 8 also shows, small firms are significantly more likely to have only one business segment, have greater idiosyncratic risk, have proportionally higher numbers of delistings, and have more negative *CFI* than larger firms. Specifically, on average, 79.08 percent of small firms have one business segment compared to 56.98 percent of large firms (t-statistic for differences between means = 65.45, significant at < 0.001 level). Small firms' idiosyncratic risk is 0.0536, compared to 0.0264 for large firms (t-statistic = 163.47, significant at the < 0.001 level). *CFI* is -0.1340 for small firms, against -0.1120 for large firms (t-statistic = -17.92, significant at the > 0.001 level). On average, 7.12 percent of small firms delist compared to 4.93 percent of large firms that delist (t-statistic = 12.35, significant at the > 0.001 level). On average, however, there is no difference between the percentages of new listings between small and large firms (t-statistic = 1.55, insignificant at the 0.10 level). Thus, small firms are less diversified, more risky, have more negative levels of investing cash flows and are more likely to be at the end of their life-cycles than larger firms. Comparing median values produce similar results and conclusions.

As the right side of Table 8, Panel A shows, there are similar mappings in firm characteristics for loss firms *vis-à-vis* profit firms. The percentage of loss firms with only one reporting segment is significantly greater than the percentage of profit firms, -75.66 percent against 65.27 percent (t-statistic = 28.43, significant at the > 0.001 level).[10] Loss firms' idiosyncratic risk is 0.0583 compared to 0.0322 for profit firms (t-statistic = -122.45, significant at the > 0.001 level).[11] Loss firms invest more heavily than profit firms; *CFI* is

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[10] We also examine the number of segments itself and find similar results. Small firms have, on average, 1.35 business segments, whereas larger firms have, on average, 2.02 business segments (t-statistic = 75.88). Loss firms, on average, have 1.47 business segments and profit firms have 1.76 business segments (t-statistic = 33.81). We also note, however, that during our time period, there was a change in reporting standards regarding segment reporting. SFAS No. 131 became effective for fiscal years beginning on or after January 1, 1998. Two outcomes of the change were that more firms reported in greater numbers of lines of businesses and that fewer firms reported just one business segment (see Street et al. 2000). These results work against our hypothesis since the percentage of firms reporting losses after 1997 rose but the percentage of firms reporting one segment dropped.

[11] This finding complements Ertimur (2005), who finds that loss firms have higher levels of information asymmetry than profit firms.

TABLE 8
Differences in the Characteristics of Small versus Large and Loss versus Profit Firms (1987–2001)

Panel A: Univariate Differences in Firm Characteristics

Variable	Statistic	Small Firms (n = 38,015)	Large Firms (n = 33,397)	Test Statistic of Difference between Means/Medians/%	Loss Firms (n = 23,912)	Profit Firms (n = 47,500)	Test Statistic of Difference between Means/Medians/%
LOSS	Proportion	0.4658	0.1858	77.81***			
SMALL	Proportion				0.7405	0.4275	77.81***
SEG1	Proportion	0.7908	0.5698	65.45***	0.7566	0.6527	28.43***
IDVOL	Mean	0.0536	0.0264	163.47***	0.0583	0.0322	122.45***
	Median	0.0473	0.0226	157.56***	0.0522	0.0275	127.08***
CFI	Mean	−0.1340	−0.1120	−17.92***	−0.1427	−0.1141	−18.33***
	Median	−0.0955	−0.0890	−6.62***	−0.0886	−0.0926	0.92
NEW	Proportion	0.0752	0.0722	1.55	0.0701	0.0756	−2.68**
DELIST	Proportion	0.0712	0.0493	12.35***	0.0835	0.0496	16.55***

Panel B: Multivariate Probit Analysis of Firm Size and Accounting Losses on Firm Characteristics (n = 71,412)

$$Pr(Variable_{it}) = \beta_0 + \beta_1 SEG1_{it} + \beta_2 IDVOL_{it} + \beta_3 CFI_{it} + \beta_4 NEW + \beta_5 DELIST_{it} + \varepsilon_{it}$$

Dependent Variable	Model	Intercept	*SEG1*	*IDVOL*	*CFI*	*NEW*	*DELIST*
SMALLFIRM	Pooled	−1.6886	0.4826	36.6054	−0.2885	0.2252	0.0309
		(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.1727)
	Fama-MacBeth	−1.7941	0.4182	40.5029	−0.4917	0.1722	0.0527
		(0.0001)	(0.0001)	(0.0001)	(0.0152)	(0.1910)	(0.0893)
LOSSFIRM	Pooled	−1.6505	0.0815	24.9768	−0.5901	0.1146	0.2201
		(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
	Fama-MacBeth	−1.7254	0.0727	28.1834	−0.6240	0.1688	0.2428
		(0.0001)	(0.0168)	(0.0001)	(0.0002)	(0.0391)	(0.0018)

(*continued on next page*)

TABLE 8 (Continued)

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*, **, *** Indicate significance at the 0.05, 0.01, and 0.0001 levels, respectively.

Variable Definitions:

SMALLFIRM = indicator variable that equals 1 if the total assets of the firm are less than the 25th percentile of total assets for NYSE firms in a given year, and 0 otherwise;
LOSSFIRM = indicator variable that equals 1 if net income is negative, and 0 otherwise;
SEG1 = indicator variable that equals 1 if the firm has a single business segment, and 0 otherwise;
IDVOL = idiosyncratic component of daily stock return volatility, estimated each firm-year by regressing daily returns on the CRSP equally weighted market return and computing the standard deviation of the residuals;
CFI = cash flows from investing activities minus R&D expenses, divided by total assets;
NEW = indicator variable that equals 1 if the firm appears for the first time on the annual industrial Compustat tape, and 0 otherwise; and
DELIST = indicator variable that equals 1 in the last year that the firm appears on the Compustat Research tape.

−0.1427 for loss firms and −0.1141 for profit firms (t-statistic = 18.33, significant at the > 0.001 level). On average, 8.35 percent firms delist in a loss year compared to 4.96 percent of firms that delist in a profitable year (t-statistic = 16.55, significant at the > 0.001 level). Comparing medians produce similar results and conclusions, with only median *CFI* not being statistically different from each other. Thus, the same characteristics that are associated with small firms also describe loss firms.

In Panel B of Table 8, we present multivariate probit analyses of both *SMALLFIRM* and *LOSSFIRM* on the same set of firm characteristics. Because firms appear repeatedly from year to year, we show both pooled regression and Fama-MacBeth coefficients and test statistics. As Panel B shows, the probit models yield similar results to the univariate analyses. *SMALLFIRM* is significantly and positively related to *SEG1* and *IDVOL* and is significantly and negatively related to *CFI*. The evidence on *NEW* and *DELIST* is mixed, with *NEW* being significantly positive for the pooled-sample results only and *DELIST* being significantly positive for the Fama-MacBeth results only. *LOSSFIRM* is significantly and positively related to *SEG1*, *IDVOL*, *NEW*, and *DELIST* and is significantly negatively related to *CFI*.

In summary, we provide some intuition behind the finding that small firms are more likely to report losses than larger firms. We show that small firms are less diversified, have higher idiosyncratic risk, more negative levels of CFI (including R&D expenditures), and are more likely to be at the end of their life-cycle than larger firms. We also show that firms with similar characteristics are more likely to record an accounting loss for the year than a profit.

VIII. SUMMARY AND CONCLUSIONS

This paper examines accounting and nonaccounting fundamentals behind accounting losses over the 50-year period 1951–2001. We examine four factors: accounting conservatism, Compustat coverage of small firms, real firm performance as measured by operating cash flows, and macroeconomic productivity. Consistent with previous studies, we find a positive relation between the frequency of firms reporting negative income over time and accounting conservatism, where accounting conservatism is defined as nonoperating accruals (Givoly and Hayn 2000). However, in the overall scheme of our analyses, accounting conservatism appears to play a relatively small role in determining the frequency of losses. Instead, we find accounting losses to be significantly related to the nonaccounting factors we examine. Our results also hold when relating ROA to the same factors and are robust to alternative definitions of accounting conservatism and business cycle variables.

Our study makes two contributions. First, we expand on extant research that examines the determinants of accounting losses. Consistent with prior studies, we document a significant association between losses and accounting conservatism. Our contribution arises by demonstrating that nonaccounting factors, most notably firm size and the business cycle, also play significant roles in explaining the incidence of accounting losses over time.

Second, our study has implications regarding the usage of accounting loss information. Accounting income, and losses in particular, are relevant for determining security valuations, bankruptcy probabilities, and abandonment options. Accounting earnings and losses are inputs in contracting, shareholder litigation, dividend policy, market-listing standards, and regulatory inquiries. By improving our understanding of how losses are generated, users and practitioners can better interpret the meaning of accounting losses.

The study has several limitations. First, our use of cash flows from operating activities as our measure of real firm performance may be imperfect due to manipulation of cash flows. The recent cases of WorldCom and Parmalat demonstrate that cash flow manipulation is a real possibility, and Roychowdhury (2004) finds empirically that firms manage earnings through manipulation of real activities that affect cash flows. However, because it appears that this phenomenon is relatively new, our use of a long time-series of data from 1951–2001 should mitigate its effects. In addition, Roychowhury's (2004) findings suggest a negative relation between accounting losses and cash flows, not a positive one, as we document. Nonetheless, the possibility of cash flow manipulation should be kept in mind when interpreting the results.

Second, we take an aggregate, market-wide approach to examining the fundamentals of accounting losses. While this research design choice has the advantage of allowing us to incorporate macroeconomic conditions into our analysis, which is one of our primary objectives, it precludes analyses and inferences regarding firm-specific variables, for example, interactions between accounting losses and firms' decisions on how to finance its investments, its dividend policy, changes to its labor force, etc. We leave these analyses to future studies.

Last, while our results clearly indicate that nonaccounting factors are significant determinants of reported accounting losses, future research might more directly address the implications of these findings. For example, should the use of loss information in security valuation, bankruptcy prediction, or credit rating determinations be conditioned on current macroeconomic productivity, firm size, or real firm performance? This is one possible avenue of inquiry that future research might explore.

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THE ACCOUNTING REVIEW
Vol. 81, No. 1
2006
pp. 207–225

Does the Form of Management's Earnings Guidance Affect Analysts' Earnings Forecasts?

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ABSTRACT: This study examines how the form of management's earnings guidance (point, narrow range, wide range) affects analysts' earnings forecasts. Results from two experiments demonstrate that: (1) guidance form has no effect on analysts' forecasts made immediately after the guidance; and (2) after the actual earnings announcement, guidance form and the relationship of the earnings guidance to actual earnings (guidance error) interact in their effect on analysts' forecasts. After the actual earnings announcement, guidance error leads to higher (lower) analysts' forecasts for firms with downwardly (upwardly) biased guidance; this effect of guidance error is magnified by a narrow range and reduced by a wide range, compared to a point estimate. These results suggest that treating the mean of the range endpoints as equivalent to a point estimate and failing to consider effects after the release of actual earnings may paint an incomplete picture of how management guidance affects analysts and investors. It also offers useful information to managers who issue earnings guidance, and presents a challenge to the psychology literature regarding the effects of information precision on judgment and decision making.

Keywords: *earnings guidance; preannouncements; management forecasts; forecast form; analyst forecasts.*

Data Availability: *Contact the authors.*

We acknowledge helpful comments from workshop participants at Cornell University, Hong Kong University of Science and Technology, Hong Kong Polytechnic University, Korea University, Singapore Management University, University of Houston, University of Illinois at Urbana–Champaign, and The University of New South Wales. We thank the financial analysts from the brokerage firm for participating in the study and Bernardine Low and Nick Seybert for their assistance.

Editor's note: This paper was accepted by Jane Kennedy, Editor.

Submitted February 2004
Accepted June 2005

I. INTRODUCTION

Prior research separately investigates the immediate effects of management earnings guidance (e.g., Pownall et al. 1993; Hirst et al. 1999) and actual earnings announcements on stock returns or investors'/analysts' judgments (e.g., Hirst and Hopkins 1998; Maines and McDaniel 2000). However, recent research on short-term management earnings guidance[1] (hereafter "guidance") indicates that this initial portrayal is incomplete. Soffer et al. (2000), Tan et al. (2002), and Miller (2004) find that investors and analysts also respond to the relationship of the earnings guidance to the subsequent actual earnings announcement (hereafter, "guidance error").[2] Consequently, the complete effect of earnings guidance cannot be observed until after the actual earnings announcement.

Some earnings guidance research investigates the effects of guidance form (e.g., point versus range). These studies find little reaction to form after the issuance of the guidance, but do not investigate effects on subsequent forecasts made after the release of actual earnings (e.g., Pownall et al. 1993; Hirst et al. 1999). However, like the effects of the earnings guidance, the effects of guidance form may not be fully evident until after the earnings announcement. More specifically, psychology research suggests a number of possible interactive effects of guidance error and guidance form that could only be observed after the actual earnings announcement.

Our study brings together research on the effects of guidance error and guidance form by investigating the effect of point, narrow range, and wide range guidance both immediately following the guidance and on subsequent forecasts made after the release of actual earnings. We conduct our tests using experiments in order to control for attributes of the firms, total earnings, the relationship between range endpoints and actual earnings, and other factors; overcome limited archival observations of upwardly biased guidance; and assess analysts' expectations about firm guidance strategies. Our two experiments discriminate among four alternative predictions derived from the accounting and psychology literatures.

In Experiment 1, 95 experienced sell-side analysts predict future quarterly and annual earnings in response to earnings guidance, and then revise their annual forecasts after receiving the actual quarterly earnings announcement. We manipulate two variables between-subjects: guidance form and guidance error. Participants receive guidance in the form of a point estimate, narrow range with actual earnings outside the range, or wide range with actual earnings inside the range. Results show that immediately after the earnings guidance is issued, analysts' forecasts follow managements' guidance, but are unaffected by form. Participants' forecast confidence reflects the precision of the guidance (point > narrow range > wide range; see Hirst et al. 1999).

On the other hand, participants' forecasts made following the actual earnings announcements (hereafter, "*re*forecasts") are jointly affected by guidance error *and* guidance form. Consistent with prior research (e.g., Tan et al. 2002), analysts' *re*forecasts are affected by the sign of guidance error; that is, *re*forecasts are higher for downwardly biased guidance (the point estimate or range mean is below actual earnings) than upwardly biased guidance (the point estimate or range mean is above actual earnings), even though actual earnings and total earnings news for the quarter are the same in all conditions. Furthermore, the effect of guidance error interacts with guidance form. When guidance is provided in the form of a range, guidance error has a larger or smaller effect on *re*forecasts than it does for a point estimate, depending on whether the range is narrow or wide. For a *narrow* range, guidance error has a *larger* effect on *re*forecasts of future earnings than it does when the guidance is made as a point estimate. For a *wide* range, guidance error has a *smaller* effect on *re*forecasts of future earnings than it does when guidance is issued as a point estimate. These effects are inconsistent with effects of earnings expectations after the guidance, which imply no impact of form, or with effects of confidence in those estimates, which imply that effects will be smallest for the wide range and largest for the point estimate.

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[1] We define a short-term earnings guidance (also called a warning or preannouncement or forecast in prior studies) as a management forecast of earnings for the current quarter issued during the three months between quarterly earnings announcement dates. To avoid confusion, we refer to all management forecasts as "guidance" and analysts' and investors' forecasts as "forecasts."

[2] The term guidance error refers to the relation between the earnings guidance and actual earnings, controlling for actual earnings and the total earnings news.

Experiment 2 allows us to discriminate between two models that are consistent with the results of Experiment 1. The first is the benchmark model, which argues that analysts' *re*forecasts are determined by whether actual earnings exceed or miss the benchmarks inherent in each guidance form. The benchmark model makes different predictions depending on whether actual earnings fall within or outside the range. The second is the range precision model where the effects of guidance error are magnified by a narrow range and reduced by a wide range, compared to a point estimate. Unlike the benchmark model, predictions of the range precision model are invariant to whether actual earnings fall within or outside the wide range. In the second experiment, actual earnings are set to fall outside of both the narrow and wide ranges. Results of experiment 2 are consistent with the range precision model, but not the benchmark model.

Our study contributes to the accounting literature on management guidance, has implications for guiding managers, and extends psychological theory. The current study furthers accounting research on management guidance strategies by indicating that analysts' reactions to guidance error are conditioned on guidance form, and demonstrating that wide ranges decrease and narrow ranges increase the effects of guidance error on *re*forecasts. It also introduces a range precision model that helps to explain observed effects. Our study suggests that prior studies that treat points and ranges equivalently, as well as those that consider ranges of varying widths to be alike, may mask the differential effects of range guidance. It also provides managers with information concerning likely analyst reaction to their choice of guidance form and any error in the guidance. Finally, our study contributes and presents a challenge to the psychology literature on the effects of information precision, in that the existing literature does not examine effects on future estimates conditional on actual outcomes. We show that the effects of range estimates depend on the width of the range, a phenomenon that has not been documented and explained in the psychology literature.

II. BACKGROUND AND HYPOTHESIS DEVELOPMENT

Guidance Error

The research literature (Soffer et al. 2000; Cotter et al. 2004) and financial press (e.g., Opdyke 2001) indicate that public management earnings guidance is a growing phenomenon. Recent research also indicates that most guidance issued within three months of the earnings announcement date arises from bad news firms and is downwardly biased (Choi and Ziebart 2002; Cotter et al. 2004).[3] Downwardly biased guidance is thought to be one cause of a second phenomenon documented in recent research: the proportion of firms that meet or just beat consensus expectations is unexpectedly high (Brown 2001; Degeorge et al. 1999; Bartov et al. 2002).

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[3] These findings are consistent with prior research examining nearer term guidance (Kasznik and Lev 1995; Soffer et al. 2000).

A number of studies directly examine how analysts, investors, and market prices react to biased guidance. Libby and Tan (1999) find that downward earnings guidance that leads to insufficient adjustment of analysts' forecasts after the guidance results in lower expectations of future earnings after the actual earnings announcement than does issuing no guidance at all. Soffer et al. (2000) present evidence that among firms that issue earnings guidance, those with upwardly biased guidance have lower returns than those with unbiased or downwardly biased guidance,[4] controlling for total quarterly earnings news. Consistent with these results, Tan et al. (2002) find that analysts predict highest future earnings for firms with downwardly biased guidance, followed by firms with unbiased guidance and upwardly biased guidance. Their debriefing results suggest that these forecasts are consistent with the analysts' beliefs about the firms' future prospects, and thus are not solely due to a cognitive error. Miller (2004) confirms Tan et al.'s (2002) results and demonstrates that M.B.A. student-investors provide the highest earnings estimates for firms with guidance that evenly split good news between the guidance and actual earnings surprises.

Guidance Form

Prior studies of guidance strategies examine either point estimates (e.g., Tan et al. 2002) or treat the mean of range guidance as a point estimate (e.g., Soffer et al. 2000). However, Cotter et al. (2004) find that 49 percent of guidance recorded by First Call between 1995 and 2001 are ranges and 23 percent are point estimates (see also Bamber and Cheon 1998; Kile et al. 1998). King et al. (1990) suggest that management will choose a guidance form to match the precision of their own information about the firm's future. Research in psychology finds a general preference for more precision over less precision (e.g., Kuhn and Budescu 1996; Kuhn et al. 1999), and that imprecise signals generally result in less extreme reactions (e.g., Curley et al. 1986; Einhorn and Hogarth 1985). Point forecasts are also perceived to be more precise than range forecasts (Wallsten et al. 1986; Highhouse 1994). This discussion suggests that point guidance is likely to evoke stronger reactions than range guidance, and narrower range guidance stronger reactions than wider ranges. However, these psychology studies invariably examine a context where participants make judgments or decisions immediately following the presentation of risky options. As a result, the contexts used in these psychology studies are somewhat different from the context we investigate here where analysts, after making a forecast following guidance (of varying precision), make a *re*forecast following the actual earnings release.

Three accounting studies examine investor reactions to longer-term point versus range management guidance. Two archival studies present conflicting findings. Baginski et al. (1993) find a stronger relationship between unexpected earnings and unexpected returns for more precise guidance, while Pownall et al. (1993) find no effect of guidance form on stock returns. Hirst et al. (1999) present experimental evidence indicating that point and range management guidance has the same effect on investors' own forecasts, and that range guidance reduces investor confidence in their predictions. Combined, these studies suggest that analysts' earnings forecasts following point and range guidance will be approximately equal, but that confidence in those forecasts will be higher for point guidance. None of the studies examines the effects of the width of the range or reactions following the actual earnings release.

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[4] This includes those firms whose actual earnings equal or exceed the guidance.

Joint Effect of Guidance Error and Guidance Form

None of the studies described in our literature review examines the effect of guidance form on analysts' or other market participants' reactions following the *actual* earnings announcement. Accordingly, the literature does not provide direct evidence concerning the primary issue addressed in this study: the role of point versus range guidance in determining the combined effect of the guidance and the actual earnings announcement (the guidance strategy). However, the literature does suggest a number of cues that might be taken from each guidance form that could affect the reaction to the subsequent earnings announcement. The first two, the expectations effect and the confidence effect models, are derived from findings in the accounting and psychology literatures on the initial reaction of decision makers to signals of varying precision (no effect on future expectations but differential confidence in those expectations). The next two, the benchmark effect and the range precision effect models, are derived from findings in the accounting and psychology literatures on the evaluative importance of relations to benchmarks and on causal attribution.

Expectations Effect

When the mean of a range is treated the same as a point estimate, as it is in many archival studies, the guidance surprise and actual earnings surprise are unaffected by form. Similarly, Hirst et al. (1999) find that investors' expectations of future earnings are the same in response to point and range guidance in their experiment. The simplest prediction of the effect of form can be made based on the assumption that if the expectations after the issuance of point and range guidance are the same, the actual earnings surprise is the same, and the reaction to the effect of actual earnings will also be the same; i.e., $\mu_{\text{point}} = \mu_{\text{narrow}} = \mu_{\text{wide}}$ (where μ_{point}, μ_{narrow}, and μ_{wide} refer to the effects of guidance error on *re*forecasts for point, narrow range, and wide range guidance, respectively).

Confidence Effect

However, consistent with theories of guidance precision (e.g., King et al. 1990), Hirst et al. (1999) find that investors show greater confidence in their forecasts following more precise point guidance than less precise range guidance (see also Wallsten et al. 1986; Highhouse 1994). If their student-participants were reacting to guidance precision, then presumably confidence would also be higher for a narrower range than a wider range. If greater confidence in forecasts following the guidance (arising from differences in guidance precision) increases the degree of surprise generated by the actual earnings announcement, then we would expect the effect of guidance error on *re*forecasts to be strongest for point guidance followed by a narrow range, and weakest for a wide range; i.e., $\mu_{\text{point}} > \mu_{\text{narrow}} > \mu_{\text{wide}}$. An inference that can be made from this prediction is that a point estimate behaves like a narrowest possible range (with zero variance).

Benchmark Effect

In contrast to the confidence effect model, the benchmark model distinguishes point and range estimates based on the benchmarks inherent in these different guidance forms. According to the benchmark model, a point estimate provides only one benchmark against which actual earnings are measured. In contrast, a range expresses expectations in terms of the upper and lower limits, each of which could serve as a benchmark. These benchmarks could influence analysts' reactions to actual earnings news conditioned on the guidance. For example, an extensive body of psychology research suggests that evaluations are made by comparisons to one or more reference points or standards (e.g., see Thibaut and Kelley 1959; Helson 1964; Kahneman and Tversky 1979; Kida and Smith 1995; Thaler 1999).

Following this literature, the evaluative reaction to the earnings guidance and announcement would be determined in part by the number of benchmarks to which they are compared, and whether these benchmarks are missed or exceeded. If we assume that point estimates have one benchmark and ranges two, this would suggest that analysts' *re*forecasts will be higher when two benchmarks are exceeded than when only one benchmark is exceeded. Analysts' *re*forecasts are also likely to be more positive where one benchmark is exceeded than where one is exceeded and one is missed, followed by one missed, and last, by two missed.

This discussion suggests the effects of range guidance *versus* point guidance should depend on the location of actual earnings relative to the limits of the range guidance. Suppose actual EPS is $0.12. Consider the following three downwardly biased guidance forms: point estimate ($0.07), narrow range ($0.05 to $0.09), and wide range ($0.01 to $0.13). In this example, actual EPS exceeds the one benchmark ($0.07) for the point estimate. The narrow range has more benchmarks (lower limit of $0.05 and upper limit of $0.09) than the point estimate, and both benchmarks are exceeded. In contrast, the wide range has the same number of benchmarks as the narrow range, but in the case of the wide range, one benchmark is exceeded (lower limit of $0.01), but one benchmark is missed (upper limit of $0.13). Based on the number of benchmarks exceeded and missed, analysts' *re*forecasts are anticipated to be highest for the narrow range (two benchmarks exceeded), followed by the point estimate (one benchmark exceeded), and then the wide range (one exceeded and one missed).

Suppose actual EPS remains at $0.12, but the guidance is upwardly biased: point estimate ($0.17), narrow range ($0.15 to $0.19), and wide range ($0.11 to $0.23). Here, the benchmark model would produce the opposite results. Analysts' *re*forecasts would be lowest for the narrow range (two benchmarks missed), followed by the point estimate (one benchmark missed), and then the wide range (one benchmark exceeded, one missed). Taken together, these two examples suggest that if the *narrow* range results in actual earnings *outside* of the range and the *wide* range *inside* the range, then it would lead to expectations that the effect of guidance error on analysts' *re*forecasts following the actual earnings announcement will be strongest in the narrow range, followed by the point estimate, and weakest in the wide range (i.e., $\mu_{\text{narrow}} > \mu_{\text{point}} > \mu_{\text{wide}}$).

However, if actual earnings fall *outside* both the *narrow* and *wide* ranges, a different set of predictions result. Recall our earlier example of downwardly biased guidance: point estimate ($0.07), narrow range ($0.05 to $0.09), and wide range ($0.01 to $0.13). Suppose actual EPS is now $0.14, which exceeds all benchmarks associated with each guidance form. Here, when actual EPS exceeds all benchmarks, analysts' *re*forecasts are expected to be highest for both narrow and wide ranges, followed by the point estimate. This is because both benchmarks (lower limit and upper limit) are exceeded for both narrow and wide ranges, and only one benchmark is exceeded for the point estimate. Thus, the effects of guidance error would be strongest for both narrow and wide ranges, followed by the point estimate (i.e., $\mu_{\text{narrow}} = \mu_{\text{wide}} > \mu_{\text{point}}$).

Range Precision Effect

We posit a fourth mechanism where precision only affects reactions to range estimates. Here, a point estimate is not treated merely as a very precise range. Instead, the width of the range either magnifies or reduces the effects of guidance error when compared to the point estimate. Specifically, when the range is *narrow*, the effects of guidance error are *larger* for range guidance than for point guidance (i.e., $\mu_{\text{narrow}} > \mu_{\text{point}}$). When the guidance has a *wide* range, the effects of guidance error are *smaller* for range guidance than for point guidance (i.e., $\mu_{\text{point}} > \mu_{\text{wide}}$). This prediction is based on attribution theory (Heider 1958; Jones and Davis 1965), and in particular, the argument that salient features, such as the width of the range, attract causal attributions (Taylor and Fiske 1975).

Specifically, wide ranges[5] are issued less frequently than narrow ranges, and infrequent events are more likely to attract attributions arising from situational factors (e.g., uncertainty of the environment) as opposed to dispositional factors (e.g., competence or ability of management) (see Kelley 1973). In the case of wide range guidance, analysts may attribute actual EPS falling short of or exceeding the guidance to a situational factor such as chance or some uncontrollable event. Attribution to chance or uncontrollable events leads to less blame or credit being accorded to managers (Tan and Lipe 1997), which should lead to less extreme analysts' EPS revisions and smaller effects of guidance error.

Attribution theory may also explain why narrow ranges attract more extreme reactions than do point estimates. For both guidance types, dispositional (e.g., competence) attributions as opposed to situational (e.g., luck) attributions are more likely to be made. However, managers who issue narrow range guidance additionally signal some uncertainty in the environment. Should actual earnings exceed the guidance, greater attribution to the managers' competence is made in the case of narrow ranges than point estimates because analysts accord more credit (i.e., higher EPS forecasts) to managers who are able to beat expectations despite experiencing a more uncertain environment. When earnings fall short of the guidance, both point and narrow range managers are blamed (i.e., lower EPS forecasts). However, narrow range managers may be blamed more. This is because analysts may perceive that although the narrow range (versus point estimates) managers may have been more aware of the uncertainty, they did not forewarn the extent of the uncertainty (by issuing a wider range) nor manage the uncertainty (e.g., by meeting or beating the guidance).

In summary, the range precision model predicts the following: compared to the effects of point guidance, more precise narrow range guidance magnifies the effects of guidance error, while less precise wide range guidance reduces the effects of guidance error ($\mu_{\text{narrow}} > \mu_{\text{point}} > \mu_{\text{wide}}$). Unlike the benchmark model, the range precision model makes the same predictions regardless of whether actual earnings fall inside or outside of the range guidance.

Summary

Given prior findings of the importance of the point guidance as a benchmark to which actual earnings are compared, coupled with the importance of causal attribution in predictive judgments, we find the benchmark model or the range precision model to be more persuasive than the other two arguments (simple expectations and confidence effects). In the sections that follow, we describe two experiments that test the effects of different guidance forms (point, narrow range, wide range). Because actual earnings naturally are more likely to fall outside (*versus* inside) a narrow range than a wide range, our first experiment focuses on range guidance that has this relation to actual earnings. The results of our first experiment are inconsistent with the predictions of the expectations effect and confidence effect models, but are consistent with the predictions of the benchmark and range precision models. As a consequence, we conduct a second experiment (involving actual earnings falling outside both narrow and wide ranges) that discriminates between the benchmark and range precision models. Table 1 presents the predictions of the models.

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[5] We intend the term "wide" to mean that the width of the range guidance is wider than is typically the case. In designing our experimental materials, we looked at a sample of range guidance. We chose four cents for the narrow range and 12 cents for the wide range. In order to keep the range mean equal to the point estimate, only even numbered ranges were considered. In the First Call database for 1999 and 2000, second quarter range guidance issued between 15 and 45 days before quarter end, four cents begins at the 64th percentile, and is the even-numbered amount between the median of three cents and mean of five cents. Only 8 percent of the examples had ranges in excess of 12 cents. We expect analysts to have knowledge of typical range widths, and, therefore, what is a wide versus a narrow range.

III. EXPERIMENT 1

Participants

Our participants are 95 sell-side analysts from a leading worldwide investment banking, trading, and brokerage firm (mean experience as an analyst = 7.6 years). Thirty-eight percent of the analysts are Chartered Financial Analysts. On average, they have worked for the participating firm for four years. Sixty-seven percent are under 40 years of age. Participants perform the experimental task during their firm's training program. The experimental sessions last an average of 58.3 minutes.

Design

Experiment 1 uses a 2 × 3 between-subjects design, with *Guidance Error* (*Downwardly biased*, *Upwardly biased*) and *Form* (*Point*, *Narrow range*, *Wide range*) as independent variables.[6] In Experiment 1, for the *narrow range*, actual earnings fall outside the range. For the *wide range*, actual earnings are inside the range. Actual earnings, and thus total earnings news, are kept constant across guidance forms.[7] Table 2, Panel A provides the details of the manipulations.

All analysts read the company's most recent financial information for the current fiscal year-to-date (as well as earlier years) and the current quarterly and annual consensus analyst forecasts. They are then exposed to earnings guidance pertaining to the current quarter,

TABLE 1
Predicted Effects of Guidance Form on the Effects of Guidance Error on Analysts' Reforecasts after the Actual Earnings Announcement

Model	Predictions[a]
Expectations effect	$\mu_{point} = \mu_{narrow} = \mu_{wide}$
Confidence effect	$\mu_{point} > \mu_{narrow} > \mu_{wide}$
Benchmark effect	
a. if the narrow range results in actual earnings outside of the range and the wide range inside the range	$\mu_{narrow} > \mu_{point} > \mu_{wide}$
b. if both the narrow and wide ranges result in actual earnings outside of the range	$\mu_{narrow} = \mu_{wide} > \mu_{point}$
Range precision effect	$\mu_{narrow} > \mu_{point} > \mu_{wide}$

[a] Where μ_{point}, μ_{narrow}, and μ_{wide} refer to the effects of guidance error on *re*forecasts for point, narrow range, and wide range guidance. A greater effect means that the effect of bias is magnified and a lesser effect means it is reduced.

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[6] Analysts also make forecasts and *re*forecasts for two subsequent time periods that consistently show the same guidance error as that in the first period. We report these multiperiod results in another paper.

[7] An alternative would be to keep the width of the range constant, and vary actual and total news. In this design, it would be impossible to differentiate whether analysts are reacting to the news content or to the proposed mechanisms.

TABLE 2
Experiment 1: Treatment Details and Mean Analysts' EPS Forecasts and Reforecasts (in $) across Experimental Conditions (standard deviations in parentheses)

Panel A: Treatment Details

Guidance Error	Point		Wide Range		Narrow Range	
	Downwardly Biased (n = 16)	Upwardly Biased (n = 15)	Downwardly Biased (n = 17)	Upwardly Biased (n = 14)	Downwardly Biased (n = 16)	Upwardly Biased (n = 17)
Consensus Quarterly Forecast	$0.24	$0.24	$0.24	$0.24	$0.24	$0.24
EPS guidance	$0.07	$0.17	$0.01 to $0.13	$0.11 to $0.23	$0.05 to $0.09	$0.15 to $0.19
Actual earnings	$0.12	$0.12	$0.12	$0.12	$0.12	$0.12
Total earnings news	$0.12	$0.12	$0.12	$0.12	$0.12	$0.12

The column heads Point, Wide Range and Narrow Range stand under the spanning head "Guidance Form".

Panel B: Analysts' Forecasts Made after Guidance

	Point Downwardly Biased	Point Upwardly Biased	Wide Range Downwardly Biased	Wide Range Upwardly Biased	Narrow Range Downwardly Biased	Narrow Range Upwardly Biased
Current quarter EPS	0.07 (0.04)	0.17 (0.06)	0.06 (0.09)	0.16 (0.11)	0.06 (0.06)	0.17 (0.08)
12-month current year EPS	0.68 (0.07)	0.73 (0.10)	0.68 (0.14)	0.72 (0.12)	0.67 (0.08)	0.75 (0.07)
12-month succeeding year EPS	0.74 (0.13)	0.82 (0.12)	0.75 (0.15)	0.80 (0.13)	0.73 (0.13)	0.84 (0.11)

Panel C: Analysts' *Re*forecasts Made after Earnings Announcement

	Point Downwardly Biased	Point Upwardly Biased	Wide Range Downwardly Biased	Wide Range Upwardly Biased	Narrow Range Downwardly Biased	Narrow Range Upwardly Biased
12-month current year EPS	0.74 (0.14)	0.67 (0.11)	0.71 (0.12)	0.70 (0.10)	0.83 (0.07)	0.65 (0.11)
12-month succeeding year EPS	0.80 (0.12)	0.72 (0.11)	0.77 (0.17)	0.76 (0.13)	0.89 (0.09)	0.69 (0.10)

This table presents the means and standard deviations of 95 experienced sell-side analysts' responses to guidance and actual earnings announcements where *Guidance Error* (*upwardly* or *downwardly biased*), and Form (*point* estimate, *range* with actual earnings *inside* the range, and *range* with actual earnings *outside* the range) are manipulated.

after which they provide quarterly and annual *forecasts*. Then, following an actual quarterly earnings announcement, they provide *re*forecasts of annual EPS.

Procedure

The case materials are based on a manufacturer of semi-conductor and telecommunication materials listed on the American Stock Exchange. This company is a profitable firm with a history of issuing guidance, and is not widely followed by analysts. It has been used to develop the materials in other related studies (Libby and Tan 1999; Tan et al. 2002). We disguise the name of the company (and call it Zetha, Inc.) to reduce the likelihood that analysts will recognize it and make inferences based on idiosyncratic information about the company not provided to other participating analysts.

In all treatment conditions, we provide and thus hold constant actual EPS for the most recent quarter prior to the guidance, along with the quarterly and 12-month analysts' consensus forecasts prior to the guidance. In addition, the preannounced quarterly EPS understates (overstates) actual second quarter EPS by the same amount ($0.05) in the *Downwardly biased (Upwardly biased) Guidance*. This controls for the magnitude of the actual earnings surprise.

We focus on the situation where total earnings news is negative because prior research discussed earlier shows that firms predominantly provide guidance related to bad news.[8] Although total earnings news is negative, Zetha is profitable. This feature reflects findings in prior research showing that profitable firms have more incentives than loss-making firms to exceed analysts' forecasts (Brown 2001).

Analysts perform the experiment on computers during their training sessions. At the start of the experiment, the introductory screen indicates that participants will be asked to analyze the information provided on computer screens that follow, and to make earnings forecasts as they would do in their normal course of business. The analysts are then asked to read the materials enclosed in an envelope (containing the background information about Zetha and its MD&A). Other than this background information, the rest of the materials are shown on computer screens. After reading the background materials, analysts click a button on the computer screen, and are shown the five-year financial summary (comprising figures pertaining to net income, sales, key financial ratios, orders, and backlogs) for the periods 1994 to 1998. This is followed by the sales and EPS for Quarter 1 of FY 1999, and consensus EPS forecast for Quarter 2 of FY 1999 ($0.24) as well as the consensus 12-month EPS forecast for FY 1999 ($0.78).

Following this, the materials analysts read vary according to their assigned experimental condition. In the *Point/Downwardly biased Guidance* condition, analysts read the following guidance:

> On May 31, 1999, the management of Zetha issued the following statement:
>
> > The company expects earnings per share for the second quarter ending June 30, 1999 to be below expectations due to weaker than expected sales. Earnings per share are estimated to be approximately $0.07 for the quarter.

Within the *Downwardly biased Guidance* conditions, analysts in the *Wide range* and *Narrow range* treatments read the same information except that the quarterly EPS guidance is estimated to be "in the range of $0.01 to $0.13" and "in the range of $0.05 to $0.09," respectively. Within the *Upwardly biased Guidance* conditions, the EPS guidance for analysts in the *Point*, *Wide range*, and *Narrow range* conditions are \$0.17, \$0.11 to \$0.23, and \$0.15 to \$0.19, respectively.

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[8] Also, Tan et al. (2002) demonstrate that the effects of guidance strategies are symmetric in sign for good and bad news firms.

Following the guidance, analysts are asked to make EPS forecasts for the quarter ending June 30, 1999, the full year ending December 31, 1999, and the full year ending December 31, 2000. They also indicate how confident they are in the accuracy of the forecasts they have made on an 11-point scale that varies in 10s from 0 (not at all confident) to 100 (extremely confident).

Analysts are next shown the following earnings announcement:

On July 15, 1999, Zetha made the following earnings announcement:

Quarter Ended	Net Sales	\$28,026
June 30, 1999	Net Income	\$ 2,535
(\$'000)	Earnings per share	\$ 0.12

The analysts are then asked to reforecast EPS for the full year ending December 31, 1999 and the full year ending December 31, 2000, and to indicate their confidence in the accuracy of these *re*forecasts. Following this, analysts complete some demographic background and debriefing questions.

Results

Manipulation and Expectations Checks

As a joint test of the *Guidance Form* and *Guidance Error* manipulations, we ask questions pertaining to the relation between actual earnings and the guidance.[9] For analysts in the *Point* condition, we ask them whether the actual earnings announcement is below, equal to, or above the earnings guidance provided by Zetha's management. All analysts in the *Point/Downwardly biased Guidance* (*Point/Upwardly biased Guidance*) condition correctly respond that the actual EPS is above (below) the EPS guidance. For analysts in the *Wide range* and *Narrow range* conditions, we ask them whether the actual earnings announcement is inside or outside the range of Zetha's earnings guidance. Again, all analysts in the *Wide range* (*Narrow range*) condition correctly respond that the actual EPS is inside (outside) the EPS range guidance.

To further test our manipulations in the range conditions, we ask analysts in the *Wide range* condition whether the actual EPS is nearer the top, in the middle, or nearer the bottom of the EPS guidance. All analysts in the *Wide range/Downwardly biased Guidance* (*Wide range/Upwardly biased Guidance*) condition correctly respond that the actual EPS is nearer the top (bottom) of the EPS range guidance. Similarly, we ask analysts in the *Narrow range* condition whether the actual EPS announced is above the top or below the bottom of the range of Zetha's earnings guidance. All participants in the *Narrow range/Downwardly biased Guidance* (*Narrow range/Upwardly biased Guidance*) condition correctly respond that the actual EPS is above the top (below the bottom) of the range of Zetha's earnings guidance. Accordingly, the experimental manipulations are deemed successful.

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[9] Analysts make forecasts and *re*forecasts for two subsequent periods. In these subsequent periods, the manipulations in Period 1 (reported in this paper) are repeated, but for a different time period (Periods 2 and 3). The manipulation check questions refer to Period 3's materials; however, analysts' responses to these questions should apply to Period 1 because Period 3's manipulation is largely a replication of that in Period 1.

To assess whether analysts expect management to prefer a particular form of earnings guidance, we also present them with the following scenario:

> Assume that on May 31st, a company estimated that its EPS for the quarter ended June 30th would be \$0.25, which was \$0.05 *below* the consensus forecast of \$0.30. If the company decided to preannounce an EPS estimate (provide guidance) for the quarterly earnings on May 31st, in what form would it likely report for the estimate?

We provide analysts with two options: point estimate or range estimate. The majority of the analysts (84 percent) indicate that management is likely to use a range estimate. This matches the empirical findings in Cotter et al. (2004), and suggests that analysts are aware of management's preference for range estimates over point estimates.

Effects of Guidance Error and Form on Analysts' Forecasts

Results for the current and succeeding year forecasts and *re*forecasts are highly similar, and year of forecast or *re*forecast never interacts with the other variables, and thus is excluded from the discussion and tables. In all of the analyses that follow, responses for the current and succeeding years' forecasts or *re*forecasts are included and treated as a repeated-measures factor. Results can be interpreted as applying to both years' responses. Descriptive statistics are provided for forecasts in Panel B and *re*forecasts in Panel C of Table 2. As illustration, the 12-month current year *re*forecasts are plotted in Figure 1.

We first test the consistency of our results with prior research findings. As in prior research on the effects of guidance form, ANOVA tests indicate that magnitudes of the forecasts made immediately after the guidance follow the pattern of the earnings guidance ($F = 39.69$; $p = .000$), and are not affected by guidance form either as a main effect or in interaction with *Guidance Error* ($p > .539$). Pairwise contrasts indicate that analysts are most confident in their forecasts in the *Point* condition, followed by the *Narrow range* condition and the *Wide range* condition (largest $p = .034$, one-tailed).

The dependent variables for our primary tests are the *re*forecasts made after receiving the actual earnings announcement. The 2 × 3 (*Guidance Error* × *Form*) ANOVA results with *re*forecasts as the dependent variables are presented in Table 3, Panel A.[10] The main effect of guidance error is significant ($F = 28.12$, $p = .000$); *re*forecasts following the actual earnings announcement are higher in the *Downwardly biased Guidance* condition than the *Upwardly biased Guidance* condition. This finding is consistent with prior research on the effects of guidance error. While guidance form has no effect on forecasts made after the guidance, the picture changes dramatically when we look at the *re*forecasts made after the actual earnings announcement. Consistent with our predictions, Table 3, Panel A indicates a highly significant *Guidance Error* by *Form* interaction in the ANOVA ($F = 10.11$, $p = .000$).[11]

Simple main effects tests for *Guidance Error* at each level of guidance form (presented in Table 3, Panel B) show that *Guidance Error* has a statistically significant effect for the *Point* guidance conditions (\$0.07 and \$0.08 per share for the current and succeeding year; $F = 5.66$, $p = .020$), which is magnified by the *Narrow range* (\$0.18 and \$0.20 per share;

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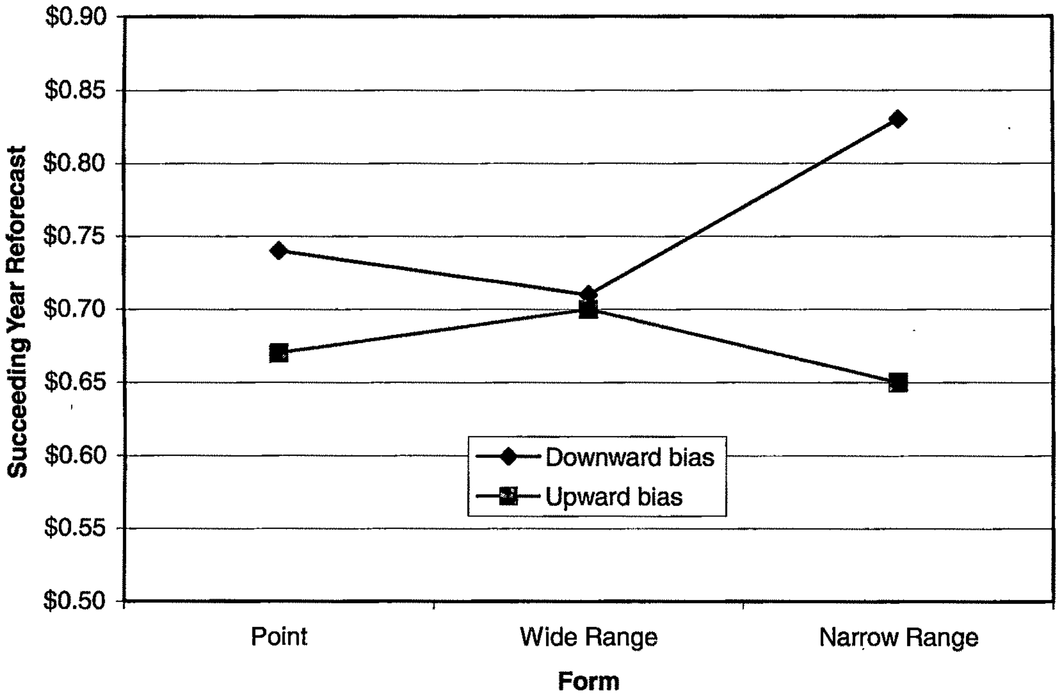
[10] ANOVA applied to the individual year responses produces highly similar results. All tests are two-tailed except where noted. These results are unaffected by inclusion of the initial forecast made after the guidance as a covariate.

[11] ANOVA tests indicate that the main effect of *Form* is marginally significant in the *Downwardly biased Guidance* condition ($F = 2.88$, $p = .067$) and statistically significant in the *Upwardly biased Guidance* condition ($F = 7.83$, $p = .001$).

FIGURE 1
Experiment 1: Analysts' Succeeding Year Reforecasts

In Experiment 1, 95 experienced sell-side analysts first make forecasts about a firm's future earnings in response to its guidance, followed by *re*forecasts after receiving the actual earnings announcement. We manipulate two between-subjects variables: *Guidance Error* (*upwardly* or *downwardly biased*) and *Form* (*point* estimate, *wide range* with actual earnings *inside* the range, and *narrow range* with actual earnings *outside* the range). *Re*forecasts plotted below are based on the 12-month current year EPS.



F = 44.12, p = .000) and reduced to insignificance by the *Wide range* guidance ($0.01 and $0.01 per share; F = .08, p = .774). Further contrast tests reveal that the effect of *Guidance Error* is larger for *Narrow range* guidance than for either *Point* (F = 8.49, p = .003, one-tailed) or *Wide range* guidance (F = 19.41, p = .000, one-tailed). The *Guidance Error* effect is marginally smaller for *Wide range* than for *Point* guidance (F = 2.18, p = .072, one-tailed). These results are inconsistent with the expectations effect and confidence effect models, and consistent with predictions of either the benchmark model or the range precision model.[12]

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[12] A contrast test indicates that after the actual earnings announcement, confidence in the *re*forecasts is actually *higher* for the *Wide range* than for the *Narrow range* conditions (F = 4.04, p = .047). In contrast, recall that before the actual earnings announcement but after the issuance of management guidance, confidence is lower for the *Wide range* than for the *Narrow range* conditions. This change in confidence before and after earnings announcement is highly significant (F = 65.24, p = .000) in a 2 (*Wide range* versus *Narrow range*) × 2 (*Before earnings announcement* versus *After earnings announcement*) ANOVA.

TABLE 3
Experiment 1 ANOVA Results: Effects of Guidance Error and Form on Analyst Reforecasts

Panel A: Two-Way ANOVA (Effect of Guidance Error and Form)

		EPS Reforecast		
Source	**df**	**Mean Squares**	**F-statistic**	**p-value**
Guidance Error	1	0.383	28.12	.000
Form	2	0.018	1.29	.280
Guidance Error × Form	2	0.138	10.11	.000
Error	89	0.0136		

Panel B: Simple Main Effects of Guidance Error at Each Level of Form

		EPS Reforecast	
Simple Main Effects	**df**	**F-statistic**	**p-value**
Bias at Point	1	5.66	.020
Bias at Narrow range	1	44.12	.000
Bias at Wide range	1	0.08	.774

Panel A presents results of an analysis of variance on the effects of *Guidance Error* (*upwardly* or *downwardly biased*) and *Form* (*point* estimate, *wide range* with actual earnings *inside* the range, and *narrow range* with actual earnings *outside* the range) on analysts' *re*forecasts.
Panel B reports the simple main effects of *Guidance Error* at each level of *Form*.
All p-values reported in ANOVA tables are two-tailed.

IV. EXPERIMENT 2

A key finding in Experiment 1 is that the effect of guidance error is greatest for guidance in the form of narrow ranges, followed by point estimates, and then wide ranges. However, as noted earlier, actual earnings fall within the wide range, but exceed the upper end of the narrow range. As a consequence, Experiment 1 cannot discriminate between our benchmark and range precision models.

Design

To disentangle these effects, we conduct Experiment 2, which is identical in design to the *Downwardly biased Guidance* condition used in Experiment 1, except that actual earnings reported after the guidance is \$0.14 instead of \$0.12. As in Experiment 1, the guidance forms are *Point* (\$0.07), *Narrow range* (\$0.05 to \$0.09), and *Wide range* (\$0.01 to \$0.13). The net effect of this new design is that in all guidance forms (point, narrow range, wide range), actual earnings now exceed the upper end of both narrow and wide ranges, as well as the point estimate. This design holds constant the sign of the distance between actual earnings and the upper end of the narrow and wide range, therefore allowing us to discriminate between the benchmark and range precision models. As indicated in Table 1, in this setting, if the benchmark model is correct, we expect *re*forecasts to follow the pattern $\mu_{narrow} = \mu_{wide} > \mu_{point}$ (where μ_{narrow}, μ_{wide}, and μ_{point} refer to EPS *re*forecasts made after guidance issued in the form of narrow range, wide range, and point estimate, respectively). If the range precision model is correct, then we expect *re*forecasts to follow the same pattern as in Experiment 1: $\mu_{narrow} > \mu_{point} > \mu_{wide}$. We do not examine the upwardly biased conditions from Experiment 1 because the effects in that experiment are symmetric for both bias directions.

Participants and Procedure

Our participants in Experiment 2 are 42 sell-side analysts at the same firm who had not participated in Experiment 1. Their profile is similar to the participants from Experiment 1: 43 percent are Chartered Financial Analysts, with 3.9 years experience with the firm, and 5.7 years as an analyst. They complete the pencil-and-paper task followed by a short debriefing questionnaire during a training session.

Results

Manipulation Checks

All participants in the *Point* (*Narrow range* and *Wide range*) conditions correctly indicate that actual earnings exceed (fall outside) the point guidance (range guidance). Consistent with results in Experiment 1, analysts' confidence in their initial forecasts are highest in the *Point*, followed by *Narrow Range*, and then *Wide range* conditions (p = .000 for all pairwise comparisons).

Effects of Guidance Error and Form on Analysts' Reforecasts

Descriptive statistics, ANOVA results, and pairwise comparisons for *re*forecasts are shown in Table 4, Panels A, B, and C, respectively. As in Experiment 1, responses for the current and succeeding years' *re*forecasts are included and treated as a repeated-measures

TABLE 4
Experiment 2 Results: Effects of Form on Analyst Reforecasts

Panel A: Descriptive Statistics—Mean (standard deviation)

	EPS Reforecast		
	Point (n = 13)	Wide Range (n = 15)	Narrow Range (n = 14)
12-month current year EPS	0.79 (0.08)	0.68 (0.09)	0.87 (0.05)
12-month succeeding year EPS	0.86 (0.09)	0.72 (0.08)	0.94 (0.12)

Panel B: One-Way ANOVA

		EPS Reforecast		
Source	df	Mean Squares	F-statistic	p-value
Form	2	0.316	24.30	.000
Error	39	0.013		

Panel C: Pairwise Comparisons

		EPS Reforecast	
	df	F-statistic	p-value
Point versus Wide	1	16.10	0.000
Point versus Narrow	1	7.46	0.009
Wide versus Narrow	1	47.90	0.000

This table presents 42 experienced sell-side analysts' responses to guidance and actual earnings announcements where we manipulate *Form* (*point* estimate, *wide range* with actual earnings *outside* the range, and *narrow range* with actual earnings *outside* the range).

Panels A, B, and C present descriptive statistics, ANOVA results, and pairwise comparison results on the effects of *Form* on analysts' *re*forecasts.

All p-values reported in ANOVA tables are two-tailed.

factor. Results show that *re*forecasts are highest for guidance in the form of *Narrow range*, next highest for *Point* estimates, and lowest for *Wide range* (succeeding year means = \$0.94, \$0.86, and \$0.72, respectively). ANOVA results indicate a statistically significant main effect of *Guidance form*, and all pairwise comparisons between different forms are statistically significant ($p < 0.009$). These results suggest that the range precision effect, as opposed to the benchmark effect, dominates in our experiments.[13]

V. CONCLUSION

The current study examines the combined effect of management guidance form (point versus range) and guidance error (relationship of earnings guidance to actual earnings) on financial analysts' subsequent earnings forecasts made after the release of actual earnings. Results from two experiments with experienced financial analysts as participants suggest that the effect of guidance error is moderated by guidance form. In Experiment 1, we examine the effect of guidance form (point, narrow range, and wide range) for both upwardly and downwardly biased guidance. In this experiment, actual earnings fall outside of the narrow range and inside the wide range. Relative to point guidance, we find that narrow range guidance *magnifies* the effects of guidance error found for point guidance, and wide range guidance *reduces* the effect of guidance error. In Experiment 2, which focuses only on downwardly biased guidance, we find that the magnifying effect of narrow ranges and the dampening effect of wide ranges are obtained even when actual earnings exceed the upper end of both ranges. We develop a range precision model, based on attribution theory, which helps to explain the observed effects.

These findings have implications for the interpretation of prior study results in the area of management guidance precision. Specifically, after the issuance of earnings guidance but before the actual earnings announcement, we replicate prior results of nonsignificant effects of guidance form on judgments and significant effects on judgment confidence (i.e., greater precision yields higher confidence; see Hirst et al. 1999). However, after the actual earnings announcement (a condition not examined in past studies), the effects change dramatically: range guidance has a greater effect on earnings *re*forecasts than point guidance if the range is narrow, and a smaller effect if the range is wide. Our results suggest that any conclusions about the effects of guidance form or precision should also consider the effect of guidance error on subsequent judgments made after the actual earnings announcement.

While the impact of guidance error on analyst forecasts depends on the form of the guidance, it is not a simple function of guidance precision. The complex relationship uncovered herein, where narrow ranges magnify the effect of guidance error and wide ranges reduce its effect, has implications for the interpretation of prior empirical research and the design of future studies. It appears that treating range guidance as equivalent to the mean of the range endpoints or treating all ranges alike regardless of width may be inappropriate. Doing so could mask important effects if a sample includes ranges of various widths, and may even explain mixed findings on the effects of range versus point guidance if samples of range guidance vary in width across studies.

The findings also provide information for managers on the likely effects of their guidance strategies. To the degree that higher analyst earnings forecasts are beneficial to firms or their managers in the short run, the results indicate a potential benefit to following a downwardly biased, narrow range guidance strategy. However, in the longer term, such guidance may create expectations for actual earnings that are hard to meet. If this is the manager's major concern, then wider range guidance can dampen any possible effects of unintended guidance error. The generality of these effects for longer term management guidance is a subject for future research.

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[13] An ANOVA test shows that analysts' confidence in their *re*forecasts is highest in the *Point*, followed by *Wide range*, and then *Narrow range* conditions ($p < .087$ for all pairwise comparisons).

In addition, psychology research on the effects of the precision of information on the attractiveness of gambles and other judgments generally finds aversion to imprecise information. However, in these psychology studies, participants make judgments or decisions immediately following the presentation of risky options. Our results suggest that future predictions and choices could change in a significant manner after the outcome of the risky choice is disclosed. We encourage future psychology research in this area to consider information precision effects after decision outcomes are known, but before subsequent choices are made.

Certain limitations of our study should be noted. Some of these relate to specific operational choices we made in designing our procedures. For example, we repeat the guidance and earlier forecasts along with the actual earnings announcement before *re*forecasts are made. This could increase the salience of the guidance and its form, and magnify their effect in our experiment compared to the actual environment. Also, experiment 2 examines a downwardly biased but not an upwardly biased guidance strategy. Although the results in Experiment 1 suggest that the effects documented in Experiment 2 would be in evidence for an upwardly biased guidance strategy, whether this is indeed the case remains an empirical question.

We identify and test only a few plausible mechanisms by which guidance form may influence analysts' forecasts, and the generality of our results may be limited by the levels chosen for our independent variables. For example, the simple counting mechanism in the benchmark model, reflecting the number of benchmarks missed or exceeded, may suffice to explain effects of relatively narrow ranges, and range precision may only kick in for extremely wide ranges. This possibility could be directly tested by decreasing the magnitude of guidance error such that two relatively narrow range widths would result in differing numbers of benchmarks exceeded or missed.

While our experiments are not designed to examine the full array of form effects on analysts' forecasts, future studies should continue to address this complex issue. As well, our findings and suggestions can be applied to future research in judgment and decision making outside of analyst forecasts. For instance, many other business decision settings, such as budgeting and performance evaluation, involve forecasts that vary in form. Contextual attributes of other settings might affect the direction and order of form effects predicted by the range precision model developed here. It is important for future research to continue examining the impact of information precision on a wide array of judgments and decisions, as the emerging evidence from our study suggests that range guidance can have multifaceted effects.

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THE ACCOUNTING REVIEW
Vol. 81, No. 1
2006
pp. 227–250

Security Analyst and Stock Market Efficiency in Anticipating Tax-Motivated Income Shifting

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ABSTRACT: In the context of the statutory tax rate reductions enacted in the Tax Reform Act of 1986, this paper investigates the degree to which capital market participants anticipate and correctly interpret temporary income effects of tax-motivated income shifting. We find evidence consistent with financial analysts' earnings forecasts failing to anticipate earnings management that shifts income from fourth quarters in higher tax rate years to immediately following first quarters of lower tax rate years. The evidence suggests that this failure is not the result of a *decision* to ignore the income shifting, but rather an *inability* to recognize temporary components of reported earnings. We also find evidence that market prices do not fully reflect the temporary income effects of tax-motivated income shifting, and that analyst inefficiency explains about half of the market inefficiency. We interpret these inefficiencies as potentially important costs of tax planning that could limit the ability of public firm managers to implement otherwise optimal tax strategies.

Keywords: *analysts' forecasts; analyst and market efficiency; taxes; earnings management.*

Data Availability: *All data used in this study are available from I/B/E/S, Compustat, and CRSP.*

I. INTRODUCTION

This study investigates the extent to which security analysts' earnings forecasts and stock prices reflect temporary income effects of tax-motivated income shifting. The Tax Reform Act of 1986 (TRA86) reduced corporate statutory income tax rates by

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This paper has benefited from comments by two anonymous reviewers, David Guenther, John Jacob, Srinivasan Rangan, Steve Rock, Frank Selto, Naomi Soderstrom, and participants at the 2002 Annual Meeting of the American Accounting Association, and workshops at Case Western Reserve University, Ohio University, and the University of Colorado at Boulder. The authors gratefully acknowledge the contribution of Thompson Financial, Inc. for providing earnings per share forecast data available through the Institutional Brokers Estimate System (I/B/E/S). This database has been provided as part of a broad academic program to encourage earnings expectations research. In addition, the authors appreciate the financial support provided by the Ohio University College of Business and the University of Colorado Leeds School of Business.

Editor's note: This paper was accepted by Terry Shevlin, Editor.

Submitted November 2002
Accepted August 2005

12 percentage points over a two-year phase-in period, and "this provided a powerful setting to assess firms' willingness to obtain tax savings by deferring earnings" (Shackelford and Shevlin 2001, 333). Prior research provides evidence of significant income shifting to take advantage of TRA86 tax rate reductions (e.g., Scholes et al. 1992; Guenther 1994; Maydew 1997; Lopez et al. 1998; Stock 2004). However, no prior study examines the impact of these income shifting strategies on market participants.

The motivation behind this study is to increase our understanding of the costs firms face when they implement tax-planning strategies. Mills et al. (1998) document significant returns to investments in tax planning and significant direct costs of pursuing tax-planning strategies. In the setting we explore, Scholes et al. (1992) find that fewer than half of their sample firms deferred income to reduce tax payments permanently. Other studies examine factors mitigating firms' incentives to engage in income shifting behavior, including debt renegotiation costs (Guenther 1994; Maydew 1997), political costs (Guenther 1994), and costs imposed on shifting firm customers (Stock 2004). However, none of these studies investigates tax-planning costs that public firms face because financial analysts and investors fail to adjust for the temporary income effects of tax-motivated income shifting.

During the TRA86 two-year tax-reduction phase-in period, firms had strong incentives to shift income from fourth quarters to following first quarters of lower tax rate fiscal years.[1] Corresponding temporary income reversals between these adjacent quarters in different fiscal years play an important role in our research design. These same firms had little or no tax incentive to shift income between tax-reduction period second and third fiscal quarters falling in the same fiscal years. We use these second and third quarter observations to control for effects of other macroeconomic time-period-specific variables that might affect forecast errors and returns during the tax-reduction period. We also use quarterly forecast errors and announcement period returns outside the tax-reduction period to control for fiscal quarter and firm-specific effects.

Our evidence suggests that financial analysts' earnings forecasts do not fully anticipate the income firms shift out of fourth quarters of high tax rate years and into first quarters of lower tax rate years. During our sample period, I/B/E/S analysts' earnings forecasts systematically overestimate firms' actual fourth quarter earnings (as reported by I/B/E/S), and underestimate the change in earnings from fourth quarters to following first quarters of lower tax rate years. In addition, we find no evidence that analysts learn about income shifting after observing fourth quarter forecast errors. First quarter forecasts released after firms' fourth quarter earnings announcements underestimate firms' actual first quarter earnings, and the absolute values of analysts' current quarter forecast errors do not decline from fourth quarters to first quarters of following lower tax rate years.

In their efforts to forecast recurring earnings, analysts might choose to issue forecasts that exclude anticipated temporary earnings effects of tax-motivated income shifting. However, our evidence suggests that this is not the case, as analysts' reports of actual earnings do not appear to exclude the temporary effects of tax-motivated income shifting. Thus, the systematic forecast errors we observe appear to be due to analysts' inability to anticipate and understand the effects of tax incentives to shift income rather than analysts' decision to ignore these effects.

We also examine stock returns, and find negative abnormal returns around firms' announcements of their fourth quarter earnings in high tax rate years, and positive abnormal returns around the same firms' announcements of earnings for first quarters of the following lower tax rate years. These results differ from Collins et al. (1998) whose evidence suggests that the market appropriately prices foreign versus domestic earnings components when U.S. multinational firms shift income into the U.S. to take advantage of lower tax rates. Collins et al. (1998) study market efficiency with respect to effects of within-year *inter*-jurisdictional income shifting; in contrast, we investigate analyst and market responses to across-year *intra*-jurisdictional income shifting. In addition, we find that analysts' earnings forecast errors explain about half of the abnormal returns associated with firms' fourth and following year first quarter earnings announcements, suggesting that investors follow analysts' lead in misinterpreting the temporary declines and subsequent increases in quarterly earnings due to tax-motivated income shifting.

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[1] For convenience, we use the phrase "tax-reduction period" to refer to the phase-in of the statutory tax rate provisions of TRA86. This does not refer to other aspects of the law, many of which effectively *increased* taxes on corporations. We discuss some of these other aspects in Section IV.

Three prior studies evaluate analyst and investor reaction to tax-related events with implications for future earnings reports. Shaw (1990) finds evidence that analysts did not revise their forecasts to reflect earnings implications of 28 firms' announcements of cash flow effects of safe-harbor leasing transactions.[2] Similarly, Chen and Schoderbek (2000) report that in 1993 analysts did not revise their earnings forecasts to reflect the expected deferred tax expense effect of a 1 percentage point increase in the statutory tax rate. The results of these two studies are consistent with analysts either underreacting to the earnings implications of these announcements or forecasting core earnings that do not include temporary earnings components. Chen et al. (2003) attempt to sort out these two explanations and find that analysts' forecasts of quarterly earnings after the tax rate change were biased in the direction of the prior quarter income effect of the firms' *one-time* deferred tax adjustments, which is inconsistent with analysts purposely ignoring this tax effect.[3]

The three studies described above investigate settings in which firms disclosed information about the cash flow or income effect of a past event (safe-harbor leases or increases in the top corporate tax rate). In contrast, we examine a tax-planning context in which firms may have difficulty communicating the income effects of their tax strategies. Mills (1998) and Mills and Sansing (2000) document the tax audit cost of disclosing book-tax differences. If firms avoid this cost with tax-reducing strategies that manage taxable income and net income together (e.g., Guenther et al. 1997), then they are not likely to disclose these strategies and risk raising "red flags" to the government.[4] In addition, information asymmetry and adverse selection issues may cause investors and analysts to doubt management assertions that lower than expected earnings result from tax-motivated earnings management strategies. Thus, it is likely that these considerations hinder and probably preclude managers from credibly signaling their income shifting strategies.

The communication issues discussed above also make our setting different from other studies of analyst and market ability to detect earnings management. For example, prior research finds that stock prices do not fully reflect less persistent accrual components of earnings (e.g., Sloan 1996; Xie 2001; Collins and Hribar 2000; Burgstahler et al. 2002; Fairfield et al. 2003) and analysts' forecasts similarly attribute too much permanence to unusually high earnings accruals (Bradshaw et al. 2001). Rangan (1998) and Teoh et al. (1998a, 1998b) find that the market does not efficiently adjust for earnings management around the time of equity offerings, and Teoh and Wong (2002) attribute these market inefficiencies to similar inefficiencies in analysts' earnings forecasts. However, managers in these settings have incentives to avoid investor and analyst detection of temporary income effects of earnings management. Burgstahler and Eames (2003) find that analysts have difficulty identifying *ex ante* which firms will manage earnings to meet targets. The authors observe that "firms have limited abilities to jointly manage forecasts and earnings" (Burgstahler and Eames 2003, 258) and call for further research examining whether analysts' forecasts reflect other forms of earnings management. In our setting where managers may wish to communicate temporary income effects of earnings management but have difficulty credibly doing so, it appears that analysts are unable to identify *ex ante* ***and*** *ex post* which firms manage earnings to mitigate taxes.

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[2] In addition, Biddle and Ricks (1988) find that analysts do not adjust their forecasts for the short-term earnings implications of firms' LIFO adoption decisions, and resulting earnings forecast errors partially explain the negative market reaction to these firms' first earnings reports using LIFO.

[3] Despite the relatively poor analyst performance in Chen et al. (2003), there is reason to believe that analysts might anticipate tax-motivated income shifting. Plumlee (2003) investigates the impact of relatively simple versus more complex aspects of TRA86 on analysts' effective tax rate forecasts. She finds correlations of the statutory tax rate reductions with analysts' effective tax rate forecast revisions, but not with analysts' effective tax rate forecast errors. This implies that analysts are aware of statutory tax rate reductions and use that information to efficiently adjust their effective tax rate forecasts. We investigate whether analysts also adjust their earnings forecasts in anticipation that firms will shift income from higher to lower tax rate years.

[4] This discussion is not intended to imply that firms are acting illegally. However, firms are likely to want to avoid undue IRS scrutiny of legal but aggressive tax planning strategies. This stems largely from the inherent uncertainty in interpreting a complex set of tax laws.

Managers whose compensation depends in part on reported income or stock prices are less likely to decide to manage earnings to capture tax savings when tax rates decline, if these tactics create uncertainty among analysts regarding firms' future earnings and potentially greater costs of capital. Thus, one implication of our findings is that failure by analysts and investors to disentangle income variation caused by tax-motivated income shifting from income variation caused by other factors (e.g., performance) can create nontax costs for firms that strategically manage earnings to reduce their tax payments. Such costs represent a friction that can reduce firms' propensity to engage in this type of tax planning.

Our results also help explain why private firms are generally more aggressive tax planners than public firms (Mills 1998; Beatty and Harris 1999). Cloyd et al. (1996) and Mills and Newberry (2001) find that privately held firms are more likely to make income-decreasing financial accounting choices to conform to and support aggressive tax-reporting decisions. Thus, lower nontax costs appear to allow private firms to be more aggressive tax planners, and Shackelford and Shevlin (2001, 378) call for further research that identifies these nontax costs. Our study provides evidence of one such cost—that aggressive tax plans of publicly held firms disrupt the time-series of their earnings and returns, potentially resulting in higher public firm cost of capital.

The organization for the rest of this paper is as follows. The next section provides background information on TRA86, describes our sample selection criteria, and provides descriptive statistics. Section III describes our empirical models, hypotheses, and results. Section IV discusses possible effects of aspects of TRA86 other than the statutory tax rate changes, and Section V summarizes and concludes the study.

II. RESEARCH SETTING, SAMPLE, AND DESCRIPTIVE STATISTICS

TRA86 provides a powerful setting to study the ability of market participants to anticipate tax-motivated income shifting for the following reasons. First, the tax rate reductions were large and prior research shows that firms responded to the incentives to shift income. Second, periods immediately before and after TRA86 contained no corporate tax rate changes, allowing us to include substantial control observations before and after our test period. Third, in any given fiscal quarter during the phase-in period, some firms expected tax rate reductions and others did not, so we can include within test period control observations. Finally, TRA86 was highly publicized and anticipated, so it is likely that if analysts and the market misunderstand incentives to shift income in this setting, then they probably misunderstand other settings with tax-motivated income shifting incentives.

Sample Period and Selection Criteria

TRA86 reduced statutory corporate tax rates from 46 percent to 34 percent over either one or two fiscal years, depending solely on the firm's fiscal year-end. The top statutory tax rate during the phase-in year(s) was 46 percent less 1 percentage point for every month after June 1987 in the current fiscal year, down to a minimum rate of 34 percent. Thus, the tax-reduction period begins with firms whose fiscal years end in July 1986. These firms saved 1 percent of all pretax income shifted from their 1986 fiscal years into their 1987 fiscal years. Their marginal tax rates were 46 percent in 1986 and 45 percent in 1987. Similarly, the tax-reduction period ends with firms whose fiscal years end in May 1988. Those firms had marginal tax rates of 35 percent in 1988 and 34 percent in 1989.

We focus on forecasts of fourth quarters of higher tax rate years and the immediately following first quarters of lower tax rate years, because we expect most income shifting of recurring items between these two fiscal quarters (Maydew 1997, 87).[5] If firms' tax-motivated earnings management tactics create income reversals over longer than one quarter horizons, then this reduces the power of our tests. Our tax-reduction period includes all fourth and following year first quarters where statutory tax rate changes provided any incentive to shift income in or out of that quarter. We refer to these as shifting firm-quarters and they include all fourth fiscal quarters ending between July 1986 and May 1988, and all first fiscal quarters ending between October 1986 and August 1988.

Our tests evaluate shifting firm-quarter forecasts with reference to control quarter forecasts. To control for time-period, firm, and fiscal quarter effects, we include control quarters both within and outside the tax-reduction period. The within tax-reduction period control quarters include second and third fiscal quarters naturally aligned in calendar time with the shifting firm-quarters. Thus, firms switch between shifting and control firms during the tax-reduction period. For example, while some firms ended their fourth fiscal quarter on June 30, 1987, with an incentive to reduce taxes by 12 percent of any income shifted into the following fiscal year, 12/31 year-end firms ended their second fiscal quarter on June 30, 1987. In this example, the 12/31 year-end firms have no tax incentive to shift income from their second to third fiscal quarter and act as a control matched in calendar time to the June 30 year-end firms. Conversely, the June 30 year-end firms act as controls for the 12/31 year-end firms during the calendar quarters ending in December 1986 and December 1987, when (in each case) the 12/31 year-end firms had incentives to reduce taxes by 6 percent of any income shifted into the following fiscal year. To also control for fiscal quarter effects, our sample includes first through fourth fiscal quarters outside the tax-reduction period, and to control for firm effects we restrict the control sample to firms also included in the shifting firm-quarter sample. The outside tax-reduction period control sample includes all fiscal quarters ending between September 1984 and June 1986, and between September 1988 and June 1990 (i.e., 22-month windows before and after the tax-reduction period).[6]

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[5] We focus on recurring items because analysts generally exclude nonrecurring items from their forecasts and actual earnings reports. Scholes et al. (1992) and Maydew (1997) document different shifting behavior for recurring sales and SG&A expenses. Due to limitations of the I/B/E/S database, we focus on aggregated earnings. However, it is unlikely that this aggregation seriously threatens our ability to detect tax-motivated income shifting, since both revenues deferred to later quarters and expenses accelerated into high tax rate quarters reduce the income of quarters with a high tax rate.

[6] September 1984, 22 months prior to the earliest tax incentive month, is as far back as we can obtain I/B/E/S consensus quarterly forecast data, and we constrain the post-TRA86 period to equal the pre-TRA86 period.

Our sample selection begins with the 52,781 observations for quarters ending September 1984 through June 1990 for which we can compute current quarter price-deflated earnings forecast errors using I/B/E/S data. Table 1 shows that several screens reduce this sample to a total of 19,272 shifting and control firm-quarters. First, since we are interested in analyst ability to anticipate income shifting between adjacent quarters, we require forecasts of both current and next quarter *EPS* (i.e., forecasts of quarters q and $q+1$ published prior to the quarter q earnings announcement). This requirement eliminates 12,605 observations. Second, our models include controls for the first and fourth lag of the forecast and actual earnings variables, and this eliminates 15,824 firm-quarters with missing lagged values of these variables. Third, our within-firm research design requires that firms included in the shifting firm-quarter sample have enough data to estimate our variables in *both* the fourth *and* immediately following first quarters of years with changing tax rates. This requirement eliminates an additional 419 firm-quarters. Finally, our requirement that the sample exclude firms from the control sample if those firms are not also represented in the

TABLE 1
Sample
Fiscal Quarters Ending September 1984 through June 1990

Panel A: Sample Selection Criteria

	Number of Observations	% of Total
I/B/E/S observations with data needed to measure current quarter price-deflated forecast errors (most recent consensus current quarter forecast, actual current quarter *EPS*, and stock price from the first I/B/E/S report containing actual current quarter *EPS*)	52,781	100.0
Observations missing next quarter forecast or actual *EPS* needed to measure forecasted and actual quarter-to-quarter *EPS* changes	(12,605)	(23.9)
Observations missing lagged values of forecast error or forecasted/actual changes in *EPS* (first and fourth lags are used as control variables in regression models)	(15,824)	(30.0)
Shifting firm-quarter observations with enough data to estimate model variables for one but not both (fourth and immediately following first) quarters affected by tax incentives to shift income	(419)	(0.8)
Delete control firm-quarters for firms not represented among shifting firm-quarter test sample	(4,661)	(8.8)
Full forecast error sample (1,467 firms)	**19,272**	**36.5**

Panel B: Distribution of 19,272 Firm-Quarter Observations

Control firm-quarter observations:	
2nd and 3rd fiscal quarters inside the tax-reduction period	2,716
1st through 4th fiscal quarters outside the tax-reduction period	12,356
Total control firm-quarter sample	**15,072**
Shifting firm-quarters observations with tax incentives to shift income from 4th to following year 1st fiscal quarters:	
4th fiscal quarters	2,100
1st fiscal quarters	2,100
Total shifting firm-quarter sample	**4,200**

shifting sample results in the loss of an additional 4,661 firm-quarters. These criteria result in a base sample of 1,467 firms and 19,272 quarters.

As shown in Table 1, Panel B the sample includes 15,072 control firm-quarters: 2,716 second and third fiscal quarters in the tax-reduction period, and 12,356 fiscal quarters outside the tax-reduction period. Panel B also shows that the sample includes 4,200 shifting firm-quarters (2,100 fourth quarters of higher tax rate years and 2,100 immediately following first firm-quarters of lower tax rate years). Of these 2,100 fourth to first quarter observation pairs, 1,457 (69 percent) belong to December year-end firms with 6 percent tax incentives to move income from the fourth quarter of 1986 (1987) into the 1987 (1988) fiscal year. The remaining 643 observation pairs belong to non-12/31 year-end firms with incentives ranging from 1 percent to 12 percent.[7]

Not all firms expecting a decrease in statutory income tax rates have incentives to shift income to reduce tax payments. Firms with net operating loss (NOL) carryforwards receive no benefit from shifting income, and this works against the power of our analyst and market efficiency tests that depend on income shifting inside the tax-reduction period. In addition, Maydew (1997) finds that firms in NOL carryback positions during the three years following enactment of TRA86 shifted income forward to increase the size of their carrybacks to pre-TRA86 high tax rate years. Thus, some observations in our post-test-period control sample are subject to similar incentives as the firms in our shifting firm-quarter sample, and this works against the power of our tests that depend on little or no income shifting outside the tax-reduction period. Due to measurement error issues in accurately identifying NOL status (e.g., Kinney and Swanson 1993), we chose to leave NOL carryforward and NOL carryback observations in the sample described in Table 1.[8]

Descriptive Statistics

Table 2 reports descriptive statistics for the forecast, actual earnings, and forecast error variables used in this study. We are interested in whether analysts anticipate tax-motivated income shifting from fourth quarters to following first quarters of lower tax rate years. From I/B/E/S summary files, we obtain both actual quarter q to quarter $q+1$ changes in *EPS* and the median I/B/E/S analyst forecast of those changes. Forecasts of *both* quarter q and quarter $q+1$ earnings come from the most recent monthly I/B/E/S report prior to the quarter q earnings announcement. All variables are deflated by stock price, obtained from the first monthly I/B/E/S report containing the firm's actual quarter q *EPS*. To mitigate outlier influence, we report trimmed means deleting all observations in the 2.5 percent tails of the distributions of any variable in Table 2. However, as described in Section III,

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[7] In analyses not reported, we did not detect significant differences between high (i.e., > 6 percent) and low (i.e., < 6 percent) tax incentive groups in terms of either firms' income shifting behaviors or analysts' ability to forecast these behaviors (perhaps due to the relatively small sample size). Therefore, our tests treat all 2,100 shifting firm-quarter pairs similarly, regardless of the size of the tax rate reduction. Our inferences are not sensitive to restricting the analyses to only 12/31 year-end firms.

[8] Following Mills et al. (2003) we identify NOL carryforward years with reference to positive values of the Compustat NOL variable (DATA52) and negative or zero values of the Compustat domestic current federal income tax variable (DATA63). We find only 822 NOL carryforward observations in our full 19,272 observation sample and only 252 of these belong to the 4,200 shifting firm-quarter subsample. Following Maydew (1997), we identify NOL carryback years with reference to negative values of the Compustat domestic current federal income tax variable (DATA63), and we find that less than 3 percent of the post-tax-reduction period control observations appear to be in NOL carryback positions. Eliminating the tax-reduction period NOL carryforward observations and post-tax-reduction period NOL carryback observations does not materially affect our results. As expected, restricting our analysis to only the 822 observation NOL carryforward subsample without incentives to shift income does not produce evidence of analyst failure to anticipate tax-motivated income shifting.

TABLE 2
Descriptive Statistics[a, b]

Panel A: Actual and Forecasted Changes in Price-Deflated *EPS* from Quarter q to Quarter $q+1$

	Means (n = 16,637)[c]			Medians (n = 19,272)		
	$\Delta EPS_{q,q+1}$	$F\Delta EPS_{q,q+1}$	$FE\Delta EPS_{q,q+1}$	$\Delta EPS_{q,q+1}$	$F\Delta EPS_{q,q+1}$	$FE\Delta EPS_{q,q+1}$
Inside TRA86 period						
1st to 2nd fiscal quarters	0.11	0.28	−0.17	0.14	0.22	−0.05
2nd to 3rd fiscal quarters	0.10**	0.23	−0.13*	0.06**	0.14	−0.05**
3rd to 4th fiscal quarters	−0.02	0.36*	−0.38	0.07	0.24**	−0.14
4th to 1st fiscal quarters	0.16**	−0.02	0.18**	0.08**	0.00	0.06**
Outside TRA86 period						
1st to 2nd fiscal quarters	0.14	0.30	−0.16	0.12	0.23	−0.08
2nd to 3rd fiscal quarters	−0.04	0.20	−0.24	0.00	0.12	−0.10
3rd to 4th fiscal quarters	−0.00	0.31	−0.31	0.03	0.19	−0.12
4th to 1st fiscal quarters	−0.03	0.00	−0.03	−0.05	0.00	−0.05

Panel B: Actual and Forecasted Price-Deflated *EPS* for Quarter q

	Means (n = 16,637)[c]			Medians (n = 19,272)		
	EPS_q	$FEPS_q$	FE_q	EPS_q	$FEPS_q$	FE_q
Inside TRA86 period						
1st fiscal quarter	1.53	1.62	−0.09**	1.50	1.49*	0.02**
2nd fiscal quarter	1.28**	1.49**	−0.21	1.37**	1.47**	−0.02
3rd fiscal quarter	1.61	1.80**	−0.19*	1.65	1.76**	−0.02*
4th fiscal quarter	1.36**	1.67**	−0.31*	1.48**	1.61**	−0.06**
Outside TRA86 period						
1st fiscal quarter	1.48	1.65	−0.17	1.52	1.56	−0.01
2nd fiscal quarter	1.66	1.83	−0.16	1.71	1.81	0.00
3rd fiscal quarter	1.65	1.91	−0.25	1.75	1.88	−0.05
4th fiscal quarter	1.62	1.87	−0.25	1.73	1.87	−0.03

(*continued on next page*)

TABLE 2 (Continued)

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*, ** Statistically different from the corresponding value outside the tax-reduction period at the 5 percent and 1 percent levels, respectively (t-test for differences in means and rank test for differences in medians).

[a] All numbers expressed as a percentage of stock price, % symbol omitted.

[b] Variable definitions: Actual ($\Delta EPS_{q,q+1}$), forecasted ($F\Delta EPS_{q,q+1}$), and error ($FE\Delta EPS_{q,q+1}$) in forecasting the change in price-deflated *EPS* from quarter *q* to quarter $q+1$; and actual (EPS_q), forecasted ($FEPS_q$), and error (FE_q) in forecasting *EPS* for quarter *q*. Forecasts are based on the consensus (median) from the most recent monthly I/B/E/S report prior to the quarter *q* earnings announcement. Actual *EPS* and stock price are taken from the first I/B/E/S report following the quarter *q* earnings announcement.

[c] The means are based on a trimmed sample that excludes observations in the 2.5 percent tails of the distribution of any variable. The trimmed sample of 16,637 observations is for descriptive purposes only. The full sample of 19,272 observations is used in all tests and analyses in the tables that follow. To alleviate outlier issues, for all further analyses the variables are transformed into ranks on a scale from −0.5 to +0.5.

we rely on rank transformation of each variable to mitigate outlier influences and use the full sample to estimate our regression models.

Table 2, Panel A shows a mean (median) increase in actual *EPS*, from fourth quarters to immediately following first quarters of lower tax rate years, equal to 0.16 (0.08) percent of stock price, which is significantly greater than the −0.03 mean (−0.05 median) price-deflated change in *EPS* from fourth to first quarters outside the tax-reduction period. In contrast, analysts' forecasts of these changes are close to zero and not significantly different from their forecasts outside the tax-reduction period. In addition, the mean (median) error in analysts' forecasts of these actual earnings changes is 0.18 (0.06) percent of price, and differs significantly from errors in forecasting earnings changes outside the tax-reduction period. In fact, the only case where analysts underestimate quarter-to-quarter earnings changes is for the changes from fourth to following year first quarters in the tax-reduction period. Analysts exhibit their normal optimism in forecasting every other quarter-to-quarter earnings change in the table. Thus, it appears that analysts do not anticipate increases in *EPS* consistent with firms shifting income from higher to lower tax rate years. Section III reports multivariate tests of this hypothesis.

Table 2, Panel B shows the mean and median price-deflated actual and forecasted *EPS* for each fiscal quarter inside and outside the tax-reduction period. The mean (median) actual fourth quarter *EPS* is 1.36 (1.48) percent of price inside the tax-reduction period, as compared to 1.62 (1.73) percent of price outside the tax-reduction period. These differences are statistically significant and consistent with firms shifting income out of fourth quarters of higher tax rate years. Inside the tax-reduction period, first fiscal quarters are the only quarters with higher mean reported *EPS* relative to corresponding fiscal quarters outside the tax-reduction period. Both the −0.31 mean and −0.06 median fourth quarter forecast errors, measured as actual minus forecasted price-deflated earnings, are more negative in tax-reduction period fourth quarters than in any other quarter inside or outside the tax-reduction period. Similarly, first fiscal quarter forecast errors inside the tax-reduction period are larger (less negative in the case of the mean and positive in the case of the median) than in any other quarter inside or outside the tax-reduction period. These descriptive statistics are consistent with analyst inability to forecast the effects of tax incentives to shift income from fourth quarters to first quarters of lower tax rate years, and this inability seems to persist even after analysts see their fourth quarter forecast errors. Section III reports multivariate tests of these hypotheses.

III. EMPIRICAL MODELS, HYPOTHESES, AND RESULTS

Analyst Anticipation of *EPS* Changes Consistent with Income Shifting to Reduce Taxes

Our first line of inquiry is to investigate whether financial analysts efficiently anticipate income shifting in response to changing statutory tax rates. We address this question using the three empirical models described below (firm *j* subscripts suppressed in all models throughout the paper). The first model assesses whether changes in firms' actual quarterly *EPS* as reported by I/B/E/S are consistent with firms shifting income from fourth to following first quarters of years with declining tax rates.

$$\begin{aligned}\Delta EPS_{q,q+1} = {} & \lambda_{01} + \lambda_{11}\Delta EPS_{q-1,q} + \lambda_{21}\Delta EPS_{q-4,q-3} + \lambda_{31}Q4T + \lambda_{41}Q1 \\ & + \lambda_{51}Q2 + \lambda_{61}Q3 + \lambda_{71}Q1T + \lambda_{81}Q2T + \lambda_{91}Q3T + \varepsilon_{q,q+1}. \qquad (1)\end{aligned}$$

The dependent variable, $\Delta EPS_{q,q+1}$ represents the price-deflated actual change in *EPS* from quarter q to quarter $q+1$, as reported by I/B/E/S, and $\Delta EPS_{q-1,q}$ ($\Delta EPS_{q-4,q-3}$) represents the first (fourth) lag of the dependent variable. *Qk* indicates whether quarter q is a *k*th fiscal quarter, $k \in \{1,2,3\}$; and *QkT* indicates whether quarter q is a *k*th fiscal quarter that falls within the tax-reduction period, $k \in \{1,2,3,4\}$. Thus, the intercept captures fourth to first quarter changes in *EPS* outside the tax-reduction period. The second subscript on the λ coefficients refers to model (1). To mitigate the influence of outliers and avoid arbitrary truncation or winsorization, we rank transform all earnings and forecast variables in all models throughout the paper. To allow normal interpretation of the signs of model coefficients, rank values (corresponding to the order of the observations in the empirical distribution of each variable) are further transformed to a uniform scale from -0.5 to $+0.5$.[9]

The coefficient λ_{31} represents incremental tax-reduction period changes in *EPS* from fourth to following year first quarters, relative to the changes from fourth to following year first quarters outside the tax-reduction period. If firms shifted income, then we expect $\lambda_{31} > 0$ and $\lambda_{31} > \lambda_{k1}$ ($k \in \{7,8,9\}$), reflecting unusually large increases in actual *EPS* from fourth to following year first fiscal quarters during the tax-reduction period. The first lag of the dependent variable controls for normal quarter-to-quarter reversals of temporary *EPS* changes, and therefore we expect $\lambda_{11} < 0$. The fourth lag controls for seasonal and firm-specific factors (e.g., growth opportunities) and therefore we expect $\lambda_{21} > 0$. We make no predictions regarding the other indicator variables, which control for differences in *EPS* changes between other adjacent fiscal quarters inside and outside the tax-reduction period.

Assuming estimates of model (1) provide evidence consistent with tax-motivated income shifting, models (2) and (3) below evaluate the degree to which analysts anticipate this behavior. Model (2) replaces the actual *EPS* change variables in model (1) with analysts' forecasts of those changes. Consensus (median) forecasts of *both* q and $q+1$ earnings come from the most recent I/B/E/S report prior to the quarter q earnings announcement. If analysts anticipate that firms shift income to reduce taxes, then we make the same predictions for the coefficients in model (2) as for model (1): i.e., $\lambda_{32} > 0$ and $\lambda_{32} > \lambda_{k2}$, $k \in \{7,8,9\}$, where the second subscript on the coefficients refers to model (2).

$$\begin{aligned} F\Delta EPS_{q,q+1} = {} & \lambda_{02} + \lambda_{12} F\Delta EPS_{q-1,q} + \lambda_{22} F\Delta EPS_{q-4,q-3} + \lambda_{32} Q4T + \lambda_{42} Q1 \\ & + \lambda_{52} Q2 + \lambda_{62} Q3 + \lambda_{72} Q1T + \lambda_{82} Q2T + \lambda_{92} Q3T + \varepsilon_{q,q+1}. \end{aligned} \tag{2}$$

Model (3) replaces the actual *EPS* change variables in model (1) with the error in analysts' forecasts of those changes, where the forecast error equals the difference between the actual and forecasted changes in *EPS* as defined in models (1) and (2) (i.e., $FE\Delta EPS = \Delta EPS - F\Delta EPS$). If analysts do not fully anticipate that firms shift income to reduce taxes, then we expect their forecasts to fall short of the actual changes in *EPS* from fourth quarters to immediately following first quarters of lower tax rate years, and we make the same predictions as for model (1) above: i.e., $\lambda_{33} > 0$ and $\lambda_{33} > \lambda_{k3}$, $k \in \{7,8,9\}$, where the second subscript on the coefficients refers to model (3).[10]

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[9] While no theory exists to guide appropriate transformation of forecast data, Abarbanell and Lehavy (2003), Cohen and Lys (2003), and Kothari et al. (2005) report that truncation or winsorization of even small portions of a dataset can influence the results. The rank transformation mitigates the influence of outliers and conforms the data to better fit the regression model's distribution assumptions, while retaining all observations.

$$\begin{aligned} FE\Delta EPS_{q,q+1} = {} & \lambda_{03} + \lambda_{13} FE\Delta EPS_{q-1,q} + \lambda_{23} FE\Delta EPS_{q-4,q-3} + \lambda_{33} Q4T + \lambda_{43} Q1 \\ & + \lambda_{53} Q2 + \lambda_{63} Q3 + \lambda_{73} Q1T + \lambda_{83} Q2T + \lambda_{93} Q3T + \varepsilon_{q,q+1}. \qquad (3) \end{aligned}$$

Table 3 reports the results of estimating models (1), (2), and (3) above. The significantly negative λ_{11} and λ_{12} coefficients in models (1) and (2), respectively, are consistent with earnings changes containing temporary components and the models effectively controlling for this general earnings characteristic. Similarly, the significantly positive λ_{21} and λ_{22} coefficients are consistent with seasonal or firm-specific effects that the models effectively control. The significantly negative λ_{13} in model (3) suggests that analysts' forecasts do not fully adjust for effects of temporary earnings changes, and the significantly positive λ_{23} is consistent with analyst underreaction to the information in the prior year's earnings change. It is also apparent that actual and forecasted earnings changes for interim quarters differ systematically from fourth quarter earnings changes, as coefficients (λ_{4m}, λ_{5m}, λ_{6m}) on all three dummy variables indicating the fiscal quarter of the year are statistically significant in all models.

With all the above-described controls in place, we find highly significant incremental increases in earnings from fourth quarters to following first quarters of lower tax rate years (t-statistic associated with $\lambda_{31} = 7.36$). While incremental tax-reduction period changes in earnings between quarters without tax incentives to shift income are also positive, λ_{31} is larger than λ_{71}, λ_{81}, and λ_{91} (t-statistics associated with $\lambda_{31} > \lambda_{71}$ and $\lambda_{31} > \lambda_{91}$ are highly significant, and the t-statistic associated with $\lambda_{31} > \lambda_{81}$ is 1.54 with a one-tailed p-value of 0.062). These results are consistent with firms moving income from fourth to following lower tax rate first quarters during the tax-reduction period. More importantly, results of estimating models (2) and (3) suggest that analysts do not anticipate these earnings changes. In model (3), the significantly positive intercept indicates that, controlling for lagged forecast errors, actual incremental earnings changes normally exceed analysts' forecasts in fourth quarters of fiscal years during our sample period. However, the coefficient λ_{33} indicates significant incremental errors in forecasting changes in earnings from fourth to following lower tax rate first quarters. As shown in the tests at the bottom of the table, these incremental forecast errors are significantly larger than incremental errors in forecasting changes in earnings from first to second ($\lambda_{33} > \lambda_{73}$), second to third ($\lambda_{33} > \lambda_{83}$), and third to fourth ($\lambda_{33} > \lambda_{93}$) fiscal quarters during the tax-reduction period. Furthermore, the coefficient λ_{32} in model (2) is not statistically significant (t-statistic = 1.00), offering no evidence that analysts anticipate tax-reduction period income shifting.

Analyst Learning about Tax-Motivated Income Shifting

While the Table 3 results suggest that analysts failed to anticipate firms' tax planning in response to TRA86, perhaps analysts' updated forecasts (conditioned on fourth quarter forecast errors) reflect learning about income shifting. Model (4) addresses this question.

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[10] A maintained assumption of the study is that, consistent with the evidence in Plumlee (2003), analysts efficiently forecast effective tax rate changes. If this assumption does not hold, then estimates of errors in forecasting effects of income shifting on changes in *EPS* are biased upward during the tax-reduction period. Inferences based on unusually low fourth quarter tax-reduction period actual earnings (Table 2) and unusually large negative errors in forecasting those earnings (Table 4) do not depend on this assumption.

TABLE 3
Analyst Anticipation of Tax-Motivated Income Shifting[a]

$$VAR_{q,q+1} = \lambda_{0m} + \lambda_{1m}VAR_{q-1,q} + \lambda_{2m}VAR_{q-4,q-3} + \lambda_{3m}Q4T + \lambda_{4m}Q1 + \lambda_{5m}Q2 + \lambda_{6m}Q3 + \lambda_{7m}Q1T + \lambda_{8m}Q2T + \lambda_{9m}Q3T + \varepsilon_{q,q+1} \quad (1)\text{–}(3)$$

		Coefficient Estimate/(t-statistic)[b]		
Model Coefficient[d]	Model Variable[c]	Model (1)[c] $VAR_{q,q+1} = \Delta EPS_{q,q+1}$	Model (2)[c] $VAR_{q,q+1} = F\Delta EPS_{q,q+1}$	Model (3)[c] $VAR_{q,q+1} = FE\Delta EPS_{q,q+1}$
λ_{0m}	$Q4$	−0.030	−0.036	0.015
		(−5.86)	(−8.11)	(2.81)
λ_{1m}	$VAR_{q-1,q}$	−0.261	−0.015	−0.206
		(−39.68)	(−2.50)	(−29.23)
λ_{2m}	$VAR_{q-4,q-3}$	0.310	0.600	0.075
		(47.21)	(103.17)	(10.75)
λ_{3m}	$Q4T$	0.056	0.007	0.062
		(7.36)	(1.00)	(7.60)
λ_{4m}	$Q1$	0.041	0.052	−0.018
		(5.79)	(8.28)	(−2.41)
λ_{5m}	$Q2$	0.016	0.033	−0.040
		(2.37)	(5.57)	(−5.44)
λ_{6m}	$Q3$	0.016	0.043	−0.047
		(2.37)	(7.36)	(−6.60)
λ_{7m}	$Q1T$	0.016	0.007	0.024
		(2.18)	(1.09)	(2.93)
λ_{8m}	$Q2T$	0.039	0.010	0.033
		(4.69)	(1.38)	(3.77)
λ_{9m}	$Q3T$	0.009	0.023	−0.009
		(1.06)	(3.15)	(−0.98)
	Adj. R^2	19.4%	38.4%	6.3%
Test of:				
$\lambda_{3m} - \lambda_{7m} > 0$		0.039	−0.001	0.038
		(3.67)	(−0.00)	(3.25)
$\lambda_{3m} - \lambda_{8m} > 0$		0.017	−0.003	0.029
		(1.54)	(−0.33)	(2.38)
$\lambda_{3m} - \lambda_{9m} > 0$		0.047	−0.016	0.071
		(4.15)	(−1.66)	(5.82)

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[a] All models are estimated with the full sample of 19,272 observations described in Table 1. To mitigate outlier influence, values of each earnings and forecast variable are transformed into ranks based on that variable's distribution and placed on a uniform scale from −0.5 to +0.5.

[b] t-statistics of 1.28, 1.65, 1.96, 2.33, and 3.00 are associated with one-tailed p-values of 10 percent, 5 percent, 2.5 percent, 1 percent, and 0.1 percent, respectively. t-statistics at the bottom of the table are the square roots of F-statistics associated with tests of the linear restrictions.

[c] Dependent variables ($VAR_{q,q+1}$):
In model (1), $VAR_{q,q+1} = \Delta EPS_{q,q+1} = (A_{q+1} - A_q)/P_q$ = the price-deflated actual change in *EPS* from quarter q to quarter $q+1$ as reported by I/B/E/S; in model (2), $VAR_{q,q+1} = F\Delta EPS_{q,q+1} = (F_{q+1} - F_q)/P_q$ = the price-deflated forecasted change in *EPS* from quarter q to quarter $q+1$; and in model (3), $VAR_{q,q+1} = FE\Delta EPS_{q,q+1} = \Delta EPS_{q,q+1} - F\Delta EPS_{q,q+1}$ = the price-deflated error in analysts' forecasted change in *EPS* from quarter q to quarter $q+1$.
Independent variables:
$VAR_{q-1,q}$ and $VAR_{q-4,q-3}$, respectively, are the first and fourth lagged values of the dependent variable; Qk = a dummy variable indicating whether quarter q refers to the kth fiscal quarter of the year, $k \in \{1,2,3\}$; and QkT = a dummy variable indicating whether quarter q refers to a kth fiscal quarter that falls within the tax-reduction period, $k \in \{1,2,3,4\}$.

[d] The second subscript, m, on the coefficients indicates the model number, $m \in \{1,2,3\}$.

$$FE_q = \delta_0 + \delta_1 FE_{q-1} + \delta_2 FE_{q-4} + \delta_3 Q4T + \delta_4 Q1 + \delta_5 Q2 + \delta_6 Q3 + \delta_7 Q1T + \delta_8 Q2T + \delta_9 Q3T + \varepsilon_q. \quad (4)$$

The dependent variable, FE_q, represents the price-deflated difference between actual and forecasted quarter q *EPS*, with consensus (median) forecasts taken from the most recent I/B/E/S report prior to the quarter q earnings announcement, and actual *EPS* and stock price taken from the first I/B/E/S report following the quarter q earnings announcement. The lagged values of the dependent variable control for analyst underreaction to earnings information (e.g., Mendenhall 1991; Abarbanell and Bernard 1992; Raedy et al. forthcoming), as well as firm-specific and fiscal quarter-specific factors affecting quarterly forecast errors.

If analysts do not fully anticipate tax-motivated income shifting, then we expect unusually low actual relative to forecasted earnings in fourth quarters of the tax-reduction period, which corresponds to $\delta_3 < 0$ in model (4). If analysts learn about income shifting from lower than expected fourth quarter earnings in a high tax rate year, then we expect $\delta_7 < -\delta_3$. That is, if analysts understand that some portion of negative fourth quarter earnings surprises are due to income shifting, they will adjust first quarter forecasts upward to reduce their first quarter forecast errors. If, upon observing fourth quarter forecast errors, analysts become fully aware of income shifting and form unbiased expectations of first quarter earnings, then we expect $\delta_7 = 0$. If instead analysts underestimate the portion of their fourth quarter forecast errors that is due to tax-motivated income shifting, then we expect unusually large positive first quarter forecast errors and $\delta_7 > 0$.

Since we restrict the shifting firm-quarter sample to firms with observations of both fourth and following year first quarter forecast errors in the tax-reduction period, and we restrict control firm-quarters to firms also represented in the shifting firm-quarter sample, the alternative hypotheses described above ($\delta_3 < 0$, $\delta_7 < -\delta_3$ and $\delta_7 > 0$) are within-firm tests.[11] To further strengthen the research design, we compare incremental inside tax-reduction period fourth and following year first quarter forecast errors to incremental inside tax-reduction period second and following third quarter forecast errors. These comparisons control for omitted variables that may affect forecasts and actual earnings during the tax-reduction period, and our alternative hypotheses become: $\delta_3 - \delta_8 < 0$ if analysts do not fully anticipate income shifting out of tax-reduction period fourth quarters; $(\delta_7 - \delta_9) < -(\delta_3 - \delta_8)$ if analysts learn from their fourth quarter forecast errors; and $\delta_7 - \delta_9 > 0$ if, after observing their fourth quarter forecast errors, analysts still do not *fully* anticipate the income firms shift into first fiscal quarters of lower tax rate years.

As shown in Table 4, the incremental forecast errors associated with fourth (first) quarters in the tax-reduction period are more negative (positive) than the incremental forecast errors associated with any other fiscal quarter. In particular, consistent with the alternative hypothesis that analysts fail to fully anticipate income shifting out of tax-reduction period fourth fiscal quarters, δ_3 is significantly less than δ_8 (t-statistic = 1.95); and consistent with the alternative hypothesis that, conditional on observing their fourth quarter forecast

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[11] As an alternative approach to the within-firm analysis, we estimated another regression model that substitutes differences between firm-specific adjacent quarter forecast errors for the dependent and lagged independent variables in model (4) above. As expected, when we estimate this model we find that, relative to other contiguous quarter pairs both within and outside the tax-reduction period, the incremental difference between firms' first and preceding fourth quarter tax-reduction period forecast errors is positive and significant (one-tailed p-value < 0.011 in all comparisons). However, this analysis does not evaluate whether analysts learned from their fourth quarter forecast errors, which is an issue we can evaluate with model (4), while at the same time retaining the within-firm nature of the tests.

TABLE 4
Analyst Learning about Tax-Motivated Income Shifting[a,b]

$$FE_q = \delta_0 + \delta_1 FE_{q-1} + \delta_2 FE_{q-4} + \delta_3 Q4T + \delta_4 Q1 + \delta_5 Q2 + \delta_6 Q3 + \delta_7 Q1T + \delta_8 Q2T + \delta_9 Q3T + \varepsilon_q \qquad (4)$$

Model Coefficient	Model Variable	Coefficient Estimate	t-statistic[c]
δ_0	$Q4$	0.0023	0.43
δ_1	FE_{q-1}	0.2032	28.92
δ_2	FE_{q-4}	0.0965	13.75
δ_3	$Q4T$	−0.0343	−4.20
δ_4	$Q1$	0.0065	0.84
δ_5	$Q2$	0.0016	0.21
δ_6	$Q3$	−0.0265	−3.73
δ_7	$Q1T$	0.0506	6.21
δ_8	$Q2T$	−0.0108	−1.21
δ_9	$Q3T$	0.0233	2.58
	Adj. R^2	6.0%	
Test of:			
$\delta_3 - \delta_8 < 0$		−0.0236	−1.95
$\delta_7 - \delta_9 > 0$		0.0273	2.25
$\delta_7 + \delta_3 < 0$		0.0163[d]	1.41[d]
$\delta_7 - \delta_9 + (\delta_3 - \delta_8) < 0$		0.0037[d]	0.22[d]

[a] Model (4) is estimated with the full sample of 19,272 observations described in Table 1. To mitigate outlier influence, values of each forecast error variable are transformed into ranks based on that variable's distribution and placed on a uniform scale from −0.5 to +0.5.

[b] Variables: $FE_q = (EPS_q - FEPS_q)/P_q$ = the price-deflated error in analysts' forecasts of quarter q EPS; $FEPS_q$ = the median analyst forecast from the most recent I/B/E/S monthly summary file report prior to the quarter q earnings announcement; EPS_q = actual earnings per share as reported by I/B/E/S; P_q = the firm's stock price taken from the first I/B/E/S monthly file containing EPS_q; Qk = a dummy variable indicating whether quarter q refers to the kth fiscal quarter of the year $k \in \{1,2,3\}$; and QkT = a dummy variable indicating whether quarter q refers to a kth fiscal quarter that falls within the tax-reduction period, $k \in \{1,2,3,4\}$.

[c] t-statistics of 1.28, 1.65, 1.96, 2.33, and 3.00 are associated with one-tailed p-values of 10 percent, 5 percent, 2.5 percent, 1 percent, and 0.1 percent, respectively. The t-statistics at the bottom of the table are the square roots of F-statistics associated with tests of the linear restrictions.

[d] Indicates opposite hypothesized sign.

errors, analysts still do not fully anticipate income shifting into first fiscal quarters of lower tax rate years, δ_7 is significantly greater than δ_9 (t-statistic = 2.25). Furthermore, we find no evidence of analyst learning about income shifting, as $\delta_7 - \delta_9$ is not significantly less than $-(\delta_3 - \delta_8)$. If anything, the opposite appears to be true; i.e., while not statistically significant, the absolute value of incremental first quarter forecast errors exceeds the absolute value of incremental fourth quarter forecast errors during the tax-reduction period [i.e., $\delta_7 > -\delta_3$ and $(\delta_7 - \delta_9) > -(\delta_3 - \delta_8)$].

Do Analysts Adjust Reported *EPS* for Tax-Motivated Income Shifting?

The tests in Tables 3 and 4 provide no evidence that analysts forecast firms' income shifting in response to declining statutory tax rates. Perhaps analysts choose to ignore these transitory income effects and exclude them from their forecasts of "core" earnings. As described in the I/B/E/S database documentation, the administrators go to significant effort to assure that the definition of firms' *EPS* that analysts forecast is consistent with adjusted actual *EPS* numbers reported in the database (also see Abarbanell and Lehavy 2002). Therefore, if analysts purposely exclude temporary earnings effects of income shifting from their forecasts, we should see some evidence of similar adjustments of actual *EPS* in the I/B/E/S database. We use the following empirical model to address this question.

$$\begin{aligned} EPSDIF_q = {} & \alpha_0 + \alpha_1 Q4T + \alpha_2 Q1 + \alpha_3 Q2 + \alpha_4 Q3 + \alpha_5 Q1T + \alpha_6 Q2T \\ & + \alpha_7 Q3T + \alpha_8 SP_q + \varepsilon_q \end{aligned} \tag{5}$$

where $EPSDIF_q$ represents firm *j*'s actual *EPS* as reported by I/B/E/S minus that firm's actual *EPS* as reported by Compustat, all deflated by price; SP_q represents Compustat-defined special items (DATA32) divided by shares used to calculate *EPS* (DATA15) (adjusted for stock splits to the same denominator applied to the stock price deflator from I/B/E/S). We assign a value of 0 when DATA32 is missing and, consistent with the transformation procedure applied to the forecast and earnings variables in models (1) through (4), values of $EPSDIF_q$ and SP_q are transformed into ranks on a uniform scale from −0.5 to +0.5.

In model (5), $EPSDIF_q$ proxies for the sign and magnitude of the adjustments analysts make for temporary nonrecurring components of the firm's reported earnings. When analysts add back negative (subtract out positive) temporary earnings components, $EPSDIF_q$ is positive (negative). Thus, we expect negative (positive) special items to create positive (negative) $EPSDIF_q$ and therefore, consistent with prior research evidence, we expect $\alpha_8 < 0$ (e.g., Philbrick and Ricks 1991; Gu and Chen 2004). If analysts identify and choose to add back (subtract out) the negative fourth quarter (positive first quarter) temporary effects of tax-motivated income shifting, then we expect $\alpha_1 > 0$ ($\alpha_5 < 0$); and controlling for macroeconomic time-period specific factors, we expect $\alpha_1 - \alpha_6 > 0$ and $\alpha_5 - \alpha_7 < 0$. Combining these hypotheses, if analysts can identify temporary income effects of tax-motivated income shifting and choose to make corresponding adjustments to actual *EPS* reports, then we expect $(\alpha_1 - \alpha_6) - (\alpha_5 - \alpha_7) > 0$.

The sample used to estimate model (5) consists of all 17,208 of the original 19,272 observations for which we could obtain the Compustat data needed to estimate model (5). As shown in Table 5, the coefficient α_8 is negative and highly significant (t-statistic = −16.7), consistent with I/B/E/S adjusting actual earnings reports for temporary income effects associated with special items. However, contrary to the predictions above, Table 5 reports negative estimates of α_1, $\alpha_1 - \alpha_6$, and $(\alpha_1 - \alpha_6) - (\alpha_5 - \alpha_7)$. Estimates of α_5 and $(\alpha_5 - \alpha_7)$ have the hypothesized sign but are insignificantly different from zero. Thus, we fail to find evidence that analysts adjust their actual *EPS* reports for tax-motivated income shifting, and this supports the inference that failure to anticipate tax-motivated income shifting (Tables 3 and 4) is due to analyst inability rather than analyst choice.[12]

Returns Analysis

Collectively, the results in Tables 3 through 5 suggest that analysts do not efficiently forecast temporary income effects associated with tax-motivated income shifting. It also

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[12] Table 5 results are also consistent with prior research indicating that analysts generally forecast managed (as opposed to pre-managed) earnings (Burgstahler and Eames 2003) and, when analysts do exclude temporary income effects from their forecasts and actuals, they generally select those items from the set of nonrecurring line items identified in management press releases (Gu and Chen 2004).

TABLE 5
Do Analysts Adjust Reports of Firms' Actual Earnings to Reflect Temporary Effects of Tax-Motivated Income Shifting?[a]

$$EPSDIF_q = \alpha_0 + \alpha_1 Q4T + \alpha_2 Q1 + \alpha_3 Q2 + \alpha_4 Q3 + \alpha_5 Q1T + \alpha_6 Q2T + \alpha_7 Q3T + \alpha_8 SP_q + \varepsilon_q \quad (5)$$

Model Coefficient	Model Variable[b]	Coefficient Estimate	t-statistic[c]
α_0	*Q4*	0.0109	1.93
α_1	*Q4T*	−0.0118	−1.39[d]
α_2	*Q1*	−0.0100	−1.26
α_3	*Q2*	−0.0199	−0.43
α_4	*Q3*	−0.0126	−2.65
α_5	*Q1T*	−0.0037	−0.97
α_6	*Q2T*	0.0089	1.71
α_7	*Q3T*	0.0122	1.31
α_8	SP_q	−0.2292	−16.70
	Adj. R^2	1.6%	
Test of:			
$\alpha_1 - \alpha_6 > 0$		−0.0207[d]	−1.65[d]
$\alpha_5 - \alpha_7 < 0$		−0.0159	−1.26
$(\alpha_1 - \alpha_6) - (\alpha_5 - \alpha_7) > 0$		−0.0048[d]	−0.26[d]

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[a] The sample includes the subset of 17,208 observations in the 19,272 observation sample described in Table 1 with the necessary Compustat data to estimate model (5). To mitigate outlier influence, values of each variable are transformed into ranks based on that variable's distribution and placed on a uniform scale from −0.5 to +0.5.

[b] Variables: $EPSDIF_q$ represents firm *j*'s actual *EPS* as reported by I/B/E/S minus that firm's actual *EPS* as reported by Compustat, all deflated by price; and SP_q represents Compustat-defined special items (DATA32) divided by shares used to calculated *EPS* (DATA15) and adjusted for stock splits to the same denominator applied to the stock price deflator from I/B/E/S. We assign a value of zero when DATA32 is missing. Qk = a dummy variable indicating whether quarter q refers to the kth fiscal quarter of the year, $k \in \{1,2,3\}$; and QkT = a dummy variable indicating whether quarter q refers to a kth fiscal quarter that falls within the tax-reduction period, $k \in \{1,2,3,4\}$.

[c] t-statistics of 1.28, 1.65, 1.96, 2.33, and 3.00 are associated with one-tailed p-values of 10 percent, 5 percent, 2.5 percent, 1 percent, and 0.1 percent, respectively. t-statistics at the bottom of the table are the square roots of F-statistics associated with tests of the linear restrictions.

[d] Indicates opposite hypothesized sign.

appears that analysts do not learn about tax-motivated income shifting from negative fourth quarter forecast errors, and so analysts' forecasts of immediately following first quarters of lower tax rate years are predictably low. Given these results, this subsection investigates the hypothesis that the returns on these same firms' equity securities reflect *investor* misunderstanding of the temporary nature of negative fourth quarter (positive first quarter) earnings surprise. Model (6) investigates this hypothesis.

$$RET_q = \beta_0 + \beta_1(CRET_q) + \beta_2(Q4T) + \beta_3(Q1) + \beta_4(Q1T) + \varepsilon_q \quad (6)$$

where RET_q is the daily compounded return during the three trading days centered on the date (from I/B/E/S) of firm *j*'s quarter q earnings announcement, quarter q represents a fourth or first fiscal quarter either inside or outside the tax-reduction period, and $CRET_q$ is the daily compounded return for a control portfolio consisting of the stocks of all sample firms for which the three-day return window used to compute RET_q (and $CRET_q$) does not fall between the second trading day before the third quarter earnings announcement and the second trading day after the following year's first quarter earnings announcement. The sample includes the subset of 7,771 (out of 9,546) first and fourth fiscal quarter observations included in the sample described in Table 1 with the necessary CRSP data to estimate model (6). Second and fourth quarter observations are included in the computation of the control variable, $CRET_q$.

$CRET_q$ controls for market-wide events unrelated to market assessment of firms' income shifting behaviors. The coefficient β_0 ($\beta_0 + \beta_3$) measures the abnormal daily announcement period return on stocks of firms announcing their earnings for fourth (first) fiscal quarters falling *outside* the tax-reduction period; β_2 (β_4) measures the incremental abnormal daily announcement period return on stocks of firms announcing their earnings for fourth (first) fiscal quarters falling *inside* the tax-reduction period. If market expectations fail to account for firms' incentives to shift income, we expect positive (negative) market surprise when firms announce earnings for tax-reduction period first (fourth) quarters, and our alternative hypothesis is $\beta_4 > \beta_2$.

As shown in Table 6, incremental daily abnormal returns around tax-reduction period first and fourth quarter earnings announcements are approximately 0.13 percent and −0.06 percent, respectively, and the difference between them is statistically significant (t-statistic = 2.55). Table 6 also shows that long and short trading positions in stocks of firms announcing tax-reduction period first and fourth quarter earnings generate, respectively, 0.11 percent and −0.10 percent daily abnormal returns, and the difference (0.21 percent) is highly significant (t-statistic = 3.58). This difference is analogous to a 0.6 percent three-day hedge portfolio return with long (short) positions taken at the end of the second day prior to firms' announcements of their first (fourth) quarter earnings during the tax-reduction period. Given the uncertainty associated with firms' earnings announcement dates, these are not implementable hedges, and they are not traditional riskless hedges, as the long and short positions are not temporally aligned. Nonetheless, these results suggest that market expectations before announcements of fourth (first) quarter earnings of higher (lower) tax rate years do not fully incorporate firms' tax-motivated income shifting incentives.

The final question we evaluate is the extent to which failure by analysts to forecast income effects of incentives to shift income contributes to these abnormal returns. To address this question, we re-estimate model (6) after adding FE_q, the error in analysts' forecasts of quarter q earnings. As shown in the far right column of Table 6, adding the forecast error variable to the model reduces the estimate of the abnormal hedge portfolio return by about 50 percent, from 0.21 percent to 0.10 percent, and the corresponding t-statistic drops from 3.58 to 1.77. These results are consistent with analyst inefficiencies in forecasting tax-motivated income shifting explaining some but not all of the abnormal returns around the announcements of earnings affected by these tax-planning activities.[13]

IV. DISCUSSION OF TRA86 PROVISIONS OTHER THAN STATUTORY TAX RATE CHANGES

TRA86 included a number of changes to the structure of the income tax law, in addition to the phase-in of the 12 percentage point statutory tax rate reduction that is the focus of our study. Some of these other provisions created income shifting incentives consistent with

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[13] These results are consistent with other investigations of the degree to which analyst forecasting inefficiencies explain market inefficiencies in valuing equity securities (e.g., Abarbanell and Bernard 1992; Shane and Brous 2001).

TABLE 6
Returns Analysis[a]

$$\begin{aligned} RET_q &= \beta_0 + \beta_1(CRET_q) + \beta_2(Q4T) + \beta_3(Q1) + \beta_4(Q1T) + \varepsilon_q \\ RET_q &= \beta_0 + \beta_1(CRET_q) + \beta_2(Q4T) + \beta_3(Q1) + \beta_4(Q1T) + \beta_5(FE_q) + \varepsilon_q \end{aligned} \tag{6}$$

		Coefficient Estimate/(t-statistic)[c]	
Coefficient	**Variable**[b]	**Model (6) without FE_q**	**Model (6) with FE_q**
β_0	*Q4*	−0.00040	−0.00027
		(−1.22)	(−0.84)
β_1	$CRET_q$	1.12911	1.12078
		(33.86)	(34.39)
β_2	*Q4T*	−0.00059	−0.00025
		(−1.14)	(−0.50)
β_3	*Q1*	0.00019	0.00004
		(0.42)	(0.08)
β_4	*Q1T*	0.00129	0.00072
		(2.47)	(1.41)
β_5	FE_q		0.01173
			(19.22)
	Adj. R^2	12.9%	16.8%
Test of $\beta_4 - \beta_2 > 0$		0.19%	0.10%
		(2.55)	(1.34)
Long position around 1st quarter earnings announcements inside tax-reduction period ($\beta_0 + \beta_3 + \beta_4$)		0.11%	0.05%
		(2.67)	(1.22)
Short position around 4th quarter earnings announcements inside tax-reduction period ($\beta_0 + \beta_2$)		−0.10%	−0.05%
		(−2.43)	(−1.31)
Hedge: $\beta_3 + \beta_4 - \beta_2$		0.21%	0.10%
		(3.58)	(1.77)

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[a] Variables: RET_q = the daily compounded return during the three trading days centered on the date (from I/B/E/S) of firm *j*'s quarter *q* earnings announcement, quarter *q* represents a fourth or first fiscal quarter either inside or outside the tax-reduction period; $CRET_q$ = the daily compounded returns for a control portfolio consisting of the stocks of all sample firms for which the three-day return window used to compute RET_q (and $CRET_q$) does not fall between the second trading day before the third quarter earnings announcement and the second trading day after the following year's first quarter earnings announcement; *Q1* = a dummy variable indicating whether quarter *q* refers to a first fiscal quarter; and *QkT* = a dummy variable indicating whether quarter *q* refers to a *k*th fiscal quarter that falls within the tax-reduction period $k \in \{1,4\}$. To mitigate outlier influence, values of the forecast error variable are transformed into ranks based on that variable's distribution and placed on a uniform scale from −0.5 to +0.5.

[b] The sample includes the subset of 7,771 (out of 9,546) first and fourth fiscal quarter observations included in the sample described in Table 1 with the necessary CRSP data to estimate model (6). Second and fourth quarter observations are included in the computation of the control variable, $CRET_q$.

[c] t-statistics of 1.28, 1.65, 1.96, 2.33, and 3.00 are associated with one-tailed p-values of 10 percent, 5 percent, 2.5 percent, 1 percent, and 0.1 percent, respectively. The t-statistics at the bottom of the table are the square roots of F-statistics associated with tests of the linear restrictions.

those created by the statutory tax rate provisions. For example, Guenther et al. (1997) assess income shifting by 66 public firms forced by TRA86 to switch to accrual accounting for tax purposes. After the forced switch, these firms had incentives to defer income in response to the tax rate decreases, so we retain these firms in our sample.

Other aspects of TRA86 increased the corporate tax base. For example, new depreciation rules extended the lives of depreciable assets and uniform capitalization rules forced firms to capitalize certain period costs into inventory. These rules, which had various effective dates, may have affected firms' inventory production and depreciable asset acquisition decisions. Such decisions may have reduced income in the fourth quarter of 1986, but the corresponding income reversal probably spanned longer horizons, relative to horizons associated with income shifting in response to the statutory tax rate changes.

Other TRA86 rules encourage firms to reduce certain expenses after 1986. For example, Collins and Shackelford (1992) document that after 1986, U.S. multinationals reduced debt (and interest expense) by issuing preferred stock to increase available foreign tax credits. Still other TRA86 provisions change the taxation of certain types of income and create or change tax credits, without clear implications for income shifting. For example, Plumlee (2003) considers provisions increasing the capital gains rate, eliminating the investment credit, extending the research credit, and making it harder for firms to claim credits for foreign taxes paid on foreign-source income.

Some aspects of TRA86 create incentives for firms to shift income in the opposite direction as the incentives created by the statutory tax rate changes. For example, the alternative minimum tax (AMT) created a "book-income" adjustment that required corporations subject to the AMT in 1987, 1988, or 1989 to include one-half of the difference between reported income and AMT income in the AMT tax base. Firms expecting to pay AMT in 1987 had incentives to accelerate financial accounting income into 1986, which works against our tests for analyst anticipation of income shifting from 1986 into 1987.

Shackelford and Shevlin (2001, 334) note that "to our knowledge, no study jointly evaluates the rate reduction incentives to realize income after 1986 and the AMT incentives to realize income in 1986." To assess the interaction of the AMT aspects with the statutory tax rate aspects of TRA86, we re-estimate Equation (4), adding terms that interact a firm's AMT susceptibility with our variables that test for efficient analyst anticipation of income shifting in response to the statutory tax rate provisions of TRA86. Following Gramlich (1991), we define AMT-susceptible firms as those in the lowest quartile of the sample distribution with respect to taxes paid as a proportion of tax expense over the three years prior to TRA86.[14] We test whether AMT susceptibility mitigates our evidence that analysts fail to anticipate income shifting from fourth quarters into first quarters of lower statutory tax rate years. We find no evidence of mitigation of the effects described in Table 4. Thus, it appears that, on average, TRA86 incentives to defer income into lower statutory tax rate fiscal years overwhelm incentives to accelerate financial accounting income to decrease the AMT.

All in all, it is difficult to draw clear predictions about firms' earnings management strategies in response to the other aspects of TRA86 described above.. However, it seems clear from our study and prior research that during the TRA86 statutory tax rate phase-in period, firms shifted income from fourth quarters of higher statutory tax rate years to first quarters of lower tax rate years. While it is possible that this income shifting occurred in response to other aspects of the tax law, it seems clear from our study that investors and analysts have difficulty anticipating and interpreting the temporary income effects of firms' tax-motivated income shifting. Furthermore, in our setting the reversing nature of the tax incentives across consecutive quarters make it unlikely that other aspects of TRA86 that stimulate tax planning with longer horizon income implications can explain the entire pattern of our results. Thus, we feel relatively safe in attributing our results to firms shifting income in response to the statutory tax rate provisions of TRA86 and analysts' earnings forecasts failing to anticipate this behavior.

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[14] Boynton et al. (1992) examine actual tax return data and find that the Gramlich (1991) procedure contains significant measurement error. However, we employ Gramlich's procedure, because he finds income shifting into 1986 in response to the AMT, whereas Boynton et al. (1992) find no significant AMT-related income shifting into 1986.

V. SUMMARY AND CONCLUSION

This paper investigates the degree to which market participants anticipate and correctly interpret temporary income effects of firms' earnings management activities during periods of declining statutory tax rates. Failure to do so reflects information problems and corresponding nontax costs that might inhibit firms from adopting otherwise optimal tax strategies. The dramatic decline in statutory corporate tax rates promulgated by the Tax Reform Act of 1986 (TRA86) and the nature of the law's two-year tax-reduction period phase-in rules provide a setting conducive to powerful tests of the ability of financial analysts to forecast and the market to efficiently price temporary income components associated with tax-motivated income shifting.

Our tests focus on differences in actual earnings, earnings forecast errors, and returns during tax-reduction period fourth versus following year first quarters, as compared to control firm-quarters. If firms shift income, then we expect unusually low earnings in fourth quarters and unusually large increases in earnings between tax-reduction period fourth and following year first quarters. If analysts do not anticipate these earnings changes, then we expect unusually large negative fourth quarter earnings forecast errors (measured as actual minus forecasted earnings), and we also expect fourth to following year first quarter earnings changes to exceed analysts' forecasts by unusually large amounts. Consistent with these predictions, our results confirm prior research findings that firms shift significant amounts of income to take advantage of declining tax rates, and we provide new evidence that financial analysts' earnings forecasts do not anticipate the temporary earnings reversals associated with tax-motivated income shifting. In additional tests, we find no evidence that analysts adjust temporary effects of tax-motivated income shifting out of their reports of firms' actual earnings. Therefore, we attribute the above results to information frictions that hamper the forecasting process.

If analysts learn about income shifting from their tax-reduction period fourth quarter forecast errors, then we expect the absolute values of current quarter forecast errors to decline from fourth to following year first quarters during tax-reduction periods. If analysts become fully cognizant of tax-motivated income shifting, then we expect unbiased first quarter forecasts following fourth quarter earnings announcements. Our results do not support these learning hypotheses, as absolute values of forecast errors do not decline and first quarter forecasts are unusually low relative to actual first quarter earnings reports.

If market expectations regarding tax-motivated income shifting mirror those of analysts, then we expect negative abnormal returns around fourth quarter tax-reduction period earnings announcements, positive abnormal returns around announcements of earnings for first quarters of the following fiscal years, and statistically significant differences between these adjacent quarter earnings announcement period abnormal returns. Our results support these hypotheses. In addition, we find that analysts' earnings forecast errors explain about half of the abnormal returns associated with firms' fourth and following year first quarter earnings announcements. Thus, analyst inefficiencies appear to explain some but not all market inefficiency in anticipating the temporary declines and subsequent increases in quarterly earnings due to tax-motivated income shifting during a period of declining statutory tax rates.

Future research could develop more refined tests based on predictions of which components of earnings firms are most likely to manage, given nuances of the tax law change. Using a database of analysts' forecasts of earnings components (e.g., Value Line), future research could investigate hypotheses regarding tax incentives to shift income between components or within components between periods. Given documented improvements since the 1980s in the integrity and breadth of analyst forecast databases (Ramnath et al. 2005), along with documented changes in the forecasting environment (Brown 1997, 2001), future research could also investigate the generalizability of our results to tax law changes that create incentives for firms to shift income to reduce taxes in more recent time periods.

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THE ACCOUNTING REVIEW
Vol. 81, No. 1
2006
pp. 251–270

Does Income Smoothing Improve Earnings Informativeness?

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ABSTRACT: This paper uses a new approach to examine whether income smoothing garbles earnings information or improves the informativeness of past and current earnings about future earnings and cash flows. We measure income smoothing by the negative correlation of a firm's change in discretionary accruals with its change in premanaged earnings. Using the approach of Collins et al. (1994), we find that the change in the current stock price of higher-smoothing firms contains more information about their future earnings than does the change in the stock price of lower-smoothing firms. This result is robust to decomposing earnings into cash flows and accruals and to controlling for firm size, growth, future earnings variability, private information search activities, and cross-sectional correlations.

Keywords: *income smoothing; future earnings response coefficient (FERC); earnings management; informativeness.*

I. INTRODUCTION

In this paper we use a new approach to investigate whether income smoothing garbles accounting earnings information or improves the informativeness of firms' reported current and past earnings about their future earnings and cash flows. Income smoothing represents managers' attempts to use their reporting discretion to "intentionally dampen the fluctuations of their firms' earnings realizations" (Beidleman 1973, 653). Although income smoothing has been widely documented for decades,[1] its effect on earnings informativeness is largely unknown. On one hand, income smoothing improves earnings informativeness if managers use their discretion to communicate their assessment of future earnings. On the other hand, income smoothing makes earnings noisier if managers intentionally distort the earnings numbers. Which effect dominates in a cross-sectional setting is an open, empirical question. Our study contributes to the literature by shedding new light on this information-versus-garbling debate.

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[1] See Beidleman (1973), Ronen and Sadan (1981), Schipper (1989), Subramanyam (1996), and Healy and Wahlen (2000).

We thank Joel Demski, Joshua Ronen, Stephen Ryan, workshop participants of the London Business School and the Columbia-NYU Joint Accounting Seminar, and two anonymous referees.

Editor's note: This paper was accepted by Terry Shevlin, Editor.

Submitted June 2005
Accepted July 2005

Although we examine managers' discretionary reporting behavior, our study differs from most earnings management studies, which focus on the costs of earnings management (Teoh et al. 1998; Marquardt and Wiedman 2005; Bartov and Mohanram 2004, among others). We focus on the benefits of discretionary behavior. Our primary contribution is to use the approach of Collins et al. (1994) (hereafter CKSS) and provide evidence that income smoothing improves the informativeness of past and current earnings about future earnings and cash flows. We do so by investigating the association between current-year stock returns and future earnings for firms with different degrees of smoothing. We refer to this association as the future earnings response coefficient (FERC).

Assuming the informational efficiency of stock price, the CKSS approach examines how much information about future earnings is reflected in the change in current stock price. This approach is superior to estimating the direct relation between a firm's future earnings and its current and past earnings for two reasons. First, although realized earnings are often used to directly predict future earnings, the earnings information can be indirectly used by investors in earnings predictions when investors combine it with information from other sources (Christensen and Demski 2003, Chapter 10). By using stock price, which aggregates all publicly available information, the CKSS approach considers both the direct and the indirect roles of realized earnings. Second, the change in (expected) future earnings may be due to a shock that has no effect on current earnings.[2] Such information will not be captured by current earnings, but will be impounded in current stock price.

Our paper is closely related to two recent studies. Subramanyam (1996) finds that returns are positively associated with contemporaneous discretionary accruals, while Hunt et al. (2000) report that income smoothing enhances the contemporaneous price-earnings relation. Both papers focus on the relation between prices (or returns) and *contemporaneous* accounting information. In contrast, we focus on the relation between returns and *future* accounting information. An enhanced relation between prices (or returns) and contemporaneous earnings could be due to lower risk and/or greater persistence rather than to increased informativeness about the future. However, FERC reflects more than just persistence. If income smoothing makes earnings more informative, then returns should reflect more information about future earnings, and the FERC should be higher for firms with greater smoothing. If income smoothing merely garbles information, then returns should reflect less future earnings information, and the FERC should be lower for firms with greater smoothing. Thus, our focus allows better assessment of the informativeness of a firm's current and past earnings about future earnings.

We measure income smoothing as the negative correlation of a firm's change in discretionary accruals with its change in pre-managed income. A more negative correlation indicates more income smoothing. Using data from post-1988 we find that firms with greater smoothing have higher FERC. This result is robust to: decomposing earnings into cash flows and accruals; controlling for firm size, growth, future earnings variability, and private information search activities (proxied by analyst following and institutional holdings); and separating loss firms from profit firms. In addition, to address potential cross-sectional correlations in the pooled regressions, we extend the data to pre-1988 so that the number of cross-sections is large enough for the Fama and MacBeth (1973) (hereafter Fama-MacBeth) analysis, and we find similar results.

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[2] An example would be an announcement of a new product that will not be commercially available until a future period.

Despite the above evidence, our findings should be interpreted with caution for two reasons. First, market efficiency is assumed in all the tests. If the equity markets are inefficient, then the interpretation of our findings is unclear. Second, because managers' discretionary behavior is unobservable, our income-smoothing measure suffers from potential measurement error problems, something that affects many other earnings management studies. We estimate discretionary accruals using the method of Kothari et al. (2005), which controls for measurement error in well- or poorly performing firms. Nevertheless, despite our attempts to ensure that the measurement error in the discretionary accruals proxy is not driving the results, we cannot rule out measurement error as an alternative explanation for our results.

The rest of this paper is organized as follows. Section II reviews previous research on the motivations and effects of income smoothing. Section III explains our research design. Section IV discusses the data and presents the main empirical results. Section V reports the robustness tests and Section VI concludes.

II. INCOME SMOOTHING: MOTIVATIONS AND EFFECTS

Income smoothing, which Arthur Levitt labeled "cookie jar" accounting in his 1998 speech, is not a new issue. Gordon (1964, 262) predicts that as long as managers have discretion over accounting choices, they smooth reported income and the rate of growth in income. His prediction was tested in several studies. By the late 1970s, evidence for income smoothing was plentiful (Beidleman 1973; Ronen and Sadan 1981). In a recent study, Graham et al. (2005) report, "An overwhelming 96.9% of the survey respondents indicate that they prefer a smooth earnings path."[3]

Recent research has enriched our understanding of managers' use of their reporting discretion, categorizing it as either (1) garbling or (2) efficient communication of private information. Managers may smooth reported income to meet the bonus target (Healy 1985) or to protect their job (Fudenberg and Tirole 1995; Arya et al. 1998). The contracting theory argues that income garbling is an equilibrium solution because the principal would otherwise pay a high premium to compensate the agent, who has the information advantage, for taking additional risk (Lambert 1984; Demski and Frimor 1999). In these circumstances, even if the contract is efficient, the communication has been garbled and thus the reported earnings are less informative about a firm's future earnings and cash flows.

In contrast, other studies view income smoothing as a vehicle for managers to reveal their private information about future earnings (Kirschenheiter and Melumad 2002; Ronen and Sadan 1981; Sankar and Subramanyam 2001; Demski 1998). Such communication could be either active or passive. For example, Kirschenheiter and Melumad (2002) show that reported earnings have dual roles. The level of reported earnings allows investors to infer the level of permanent future cash flows. The fluctuations of reported earnings reduce investors' confidence in the inferred permanent component. The dual roles cause managers to smooth earnings.[4] Using Spence's (1973) signaling framework, Ronen and Sadan (1981) argue that only firms with good future prospects smooth earnings because borrowing from the future could be disastrous to a poorly performing firm when the problem explodes in the near term.

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[3] The most popular motivations for income smoothing cited in this field study are (1) to lower firm risk perceived by investors, (2) to negotiate better terms of trade with customers and suppliers, and (3) to convey future growth prospects to investors.

[4] Trueman and Titman (1988) find that firms smooth earnings to reduce the cost of borrowing and to favorably affect the terms of trade with suppliers and customers.

Private information about future earnings can also be communicated passively. Sankar and Subramanyam (2001) demonstrate that managers smooth income to smooth consumption and that in so doing they reveal private information about future earnings. Demski (1998) shows that, even in the absence of an incentive, future earnings are partially communicated in efficient contracting as long as managers use future earnings information to decide whether they smooth current earnings. Whether information is communicated actively or passively, income smoothing could make firms' current and past earnings more informative about future earnings and cash flows.

Note that the garbling-versus-information views lead to diametrically opposite predictions. If income smoothing is merely garbling, then earnings of firms that experience more smoothing should be less informative about future earnings. If income smoothing is used to convey private information, then the relation between current (including past) earnings and future earnings should be strengthened and, for the reason to be explained in the next section, the FERC is expected to be higher as well. Which effect dominates in a cross-sectional setting is an unanswered, empirical question that we address.

Two previous empirical studies are closely related to ours. Using the cross-sectional Jones (1991) model, Subramanyam (1996) finds that returns are positively associated with contemporaneous discretionary accruals, and that discretionary accruals are positively associated with future earnings and operating cash flows, implying that discretionary accruals convey information about firms' future prospects. He also finds that the correlation between discretionary accruals and pre-discretionary income is negative, concluding that firms engage in income smoothing. Hunt et al. (2000) find that income smoothing enhances the contemporaneous price-earnings relation, suggesting that income smoothing improves earnings informativeness. Both papers focus on the relation between prices or returns and *contemporaneous* accounting information. As we explained in Section I, we adopt a different approach that focuses on the relation between returns and future accounting information.

III. RESEARCH DESIGN

In this section we explain how we measure income smoothing, argue why the FERC captures earnings informativeness about future earnings, and present our primary and supplementary econometric models.

Income-Smoothing Measure

Income smoothing is defined as "an attempt on the part of the firm's management to reduce abnormal variations in earnings to the extent allowed under sound accounting and management principles" (Beidleman 1973, 653). Following Myers and Skinner (2002) and Leuz et al. (2003), we measure income smoothing by the negative correlation between the change in discretionary-accruals proxy (ΔDAP) and the change in pre-discretionary income (ΔPDI). This measure assumes that there is an underlying pre-managed income series and that managers use discretionary accruals to make the reported series smooth. More income smoothing is evident in a more negative correlation between ΔDAP and ΔPDI.

To estimate discretionary accruals, we use the cross-sectional version of the Jones model, modified by Kothari et al. (2005):

$$Accruals_t = \mathrm{a}(1/Assets_{t-1}) + \mathrm{b}\Delta Sales_t + \mathrm{c}PPE_t + \mathrm{d}ROA_t + \mu_t. \qquad (1)$$

In Regression (1), the total accruals (*Accruals*); change in sales ($\Delta Sales$); and gross property, plant, and equipment (*PPE*) are each deflated by the beginning-of-year total assets

(*Assets*).[5] Return on assets (*ROA*) is added as an additional control variable, because previous research finds that the Jones model is misspecified for well-performing or poorly performing firms (Dechow et al. 1995; Kothari et al. 2005).

To employ a large number of observations, we estimate the regression on all firms in the same industry (two-digit SIC) each year. The non-discretionary accruals (*NDAP*) are the fitted values of Regression (1) and the discretionary accruals (*DAP*) are the deviations of actual accruals from *NDAP*. The pre-discretionary income (*PDI*) is calculated as net income minus discretionary accruals ($PDI = NI - DAP$).

The income-smoothing measure is the correlation between the change in discretionary accruals and the change in pre-discretionary income: $Corr(\Delta DAP, \Delta PDI)$, using the current year's and past four years' observations. The use of five observations is a trade-off between a sufficiently long time-series for the income-smoothing measure and a large sample to test the model. We use annual data because there is much evidence that firms smooth fiscal-year earnings and that fourth-quarter reporting is distinctively different from that of other quarters (Jacob and Jorgensen 2003; Das and Shroff 2002). To control for industry and time effects, we use a firm's reversed fractional ranking of income smoothing (between 0 and 1) within its industry-year (two-digit SIC) and refer to it as *IS*. As a result, firms with a more negative correlation receive a higher income-smoothing ranking.[6]

FERC and Earnings Informativeness

Figure 1 illustrates the relation between FERC and earnings informativeness.[7] Because business operating cycles are continuous, when a firm gradually realizes its current earnings, it has certain private knowledge about the future earnings.[8] The more information a firm has about the future, the more successfully it can smooth its income series. Consequently, information about future earnings is revealed by a firm's reporting behavior well before the earnings are recognized. The information is reflected in the change in current stock price, which aggregates the information with other sources of public signals through the force of market arbitrage and in the process of price discovery. Thus, the change in current stock price captures the change in investors' expectation for future earnings. The strength of this relation is measured by the FERC in the CKSS framework.

Primary and Supplementary Models

The CKSS framework has its theoretical underpinning in the discounted cash flows valuation model. By assuming that investors' revisions in dividend expectations are fully summarized by their revisions in earnings expectations, CKSS model the return-earnings relation as Regression (2):

$$R_t = \alpha_0 + \beta_1 UX_t + \sum_{k=1}^{3} \gamma_k \Delta E_t(X_{t+k}) + \varepsilon_t \tag{2}$$

---

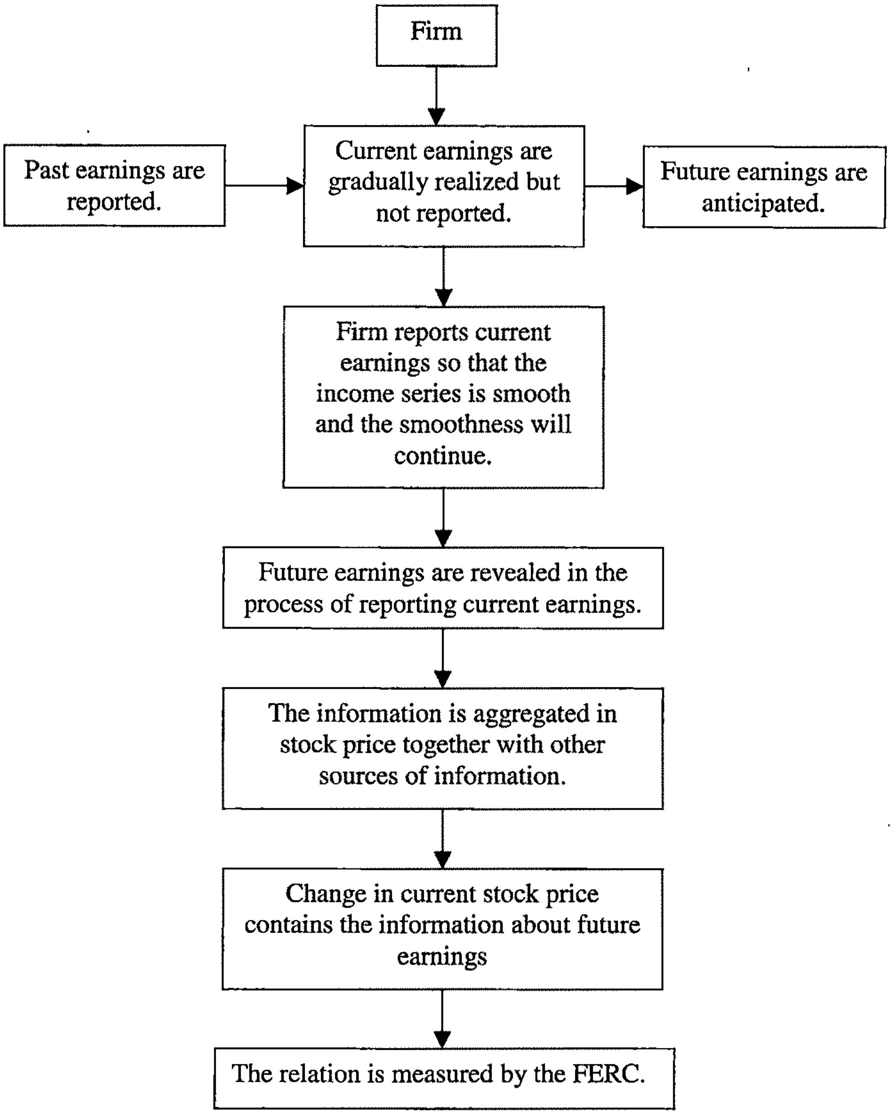
[5] "Accruals" are net income minus CFO, where CFO is obtained from the cash flow statements. "Net income," "CFO," "Sales," "PPE," and "Assets" are the variables Data18, Data308, Data12, Data7, and Data6 in the Compustat's combined industry annual data file, respectively.

[6] A fractional ranking is the raw rank divided by the number of observations. For example, the fractional rankings of 1 and 10 among the numbers 1 to 10 are 0.1 and 1, respectively.

[7] We thank an anonymous referee for suggesting Figure 1.

[8] Although earnings are reported quarterly, the information about earnings arrives at the market continuously (Ball and Brown 1968, Figure 1).

FIGURE 1
The Relation between the FERC and Earnings Informativeness in the Presence of Income Smoothing


Firm
Past earnings are reported.
Current earnings are gradually realized but not reported.
Future earnings are anticipated.
Firm reports current earnings so that the income series is smooth and the smoothness will continue.
Future earnings are revealed in the process of reporting current earnings.
The information is aggregated in stock price together with other sources of information.
Change in current stock price contains the information about future earnings
The relation is measured by the FERC.

where R_t is the ex-dividend annual stock return for Year t, UX_t is the difference between the realized earnings for Year t and what was expected at the beginning of the year, X_{t+k} is the reported earnings for Year $t+k$, and $\Delta E_t(X_{t+k})$ is the change in expectations between the end and beginning of Year t for Year $t+k$ earnings.[9] Here, β_1 is the ERC, γ_k is the FERC for Year $t+k$, and both are predicted to be positive.

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[9] By "Year," we mean "Fiscal Year" throughout the paper.

Because investors' earnings expectations are unobservable, implementing the model requires the use of proxies. CKSS use the reported earnings for Year $t-1$ as the proxy for the expectation component of UX_t. For $\Delta E_t(X_{t+k})$, CKSS use the realized earnings for Year $t+k$ as a proxy for the expectation formed at the end of Year t, and use past earnings to form an expectation at the beginning of Year t. To reduce the measurement error problem in using realized earnings (Year $t+k$) for expected earnings (expectation formed at the end of Year t), CKSS include future returns. The logic is that if realized earnings are higher (lower) than expectation, stock price should increase (decrease) accordingly from Years $t+1$ to $t+k$. This positive correlation leads to a negative loading on the future returns variable in the regression.

CKSS use earnings changes as the independent variables, implicitly assuming that annual earnings follow a random walk. Lundholm and Myers (2002) use the levels of past, current, and future earnings to allow for a more general form of earnings expectations model. To increase the power of test, Lundholm and Myers (2002) combine the three future years' earnings into variable X_{t3} and the three future years' returns into R_{t3}. As a result, we implement the CKSS approach by Regression (3).

$$R_t = b_0 + b_1 X_{t-1} + b_2 X_t + b_3 X_{t3} + b_4 R_{t3} + \varepsilon_t \tag{3}$$

In Regression (3), X_{t-1} and X_t are the earnings per share (EPS) for Years $t-1$ and t, respectively, and X_{t3} is the sum of EPS for Year $t+1$ to $t+3$. All the EPS variables are the basic EPS excluding extraordinary items (Compustat Data58), adjusted for stock splits and stock dividends, and, according to Christie (1987), deflated by the stock price at the beginning of Year t. R_{t3} is the aggregate stock return in Year $t+1$ to $t+3$ with annual compounding. The coefficient on past earnings (b_1) is predicted to be negative, the ERC (b_2) is predicted to be positive, the FERC (b_3) is predicted to be positive, and the coefficient on future returns (b_4) is predicted to be negative.

To address our research question, we expand the above regression by adding the income-smoothing measure IS and its interactions with the existing independent variables. Regression (4) is our primary empirical model:

$$\begin{aligned} R_t = {} & b_0 + b_1 X_{t-1} + b_2 X_t + b_3 X_{t3} + b_4 R_{t3} + b_5 IS_t + b_6 IS_t * X_{t-1} + b_7 IS_t * X_t \\ & + b_8 IS_t * X_{t3} + b_9 IS_t * R_{t3} + \varepsilon_t \end{aligned} \tag{4}$$

We estimate Regression (4) on pooled cross-sectional, time-series data. If the dominating effect of income smoothing is to convey information about future earnings, then the coefficient on $IS_t * X_{t3}$ should be positive. If the garbling effect of income smoothing dominates, then earnings would be less informative and thus the coefficient is expected to be negative.

As we explained in Section I, using stock price has an advantage over estimating the relation between current earnings and future earnings. Despite the difference, the two tests are related. If income smoothing improves earnings informativeness, then it must strengthen the relation between future earnings and current earnings—i.e., it must increase earnings persistence. To confirm this, we estimate the relation between current and future earnings in Regression (5):

$$EPS_{t3} = a_0 + a_1 EPS_t + a_2 IS_t + a_3 IS_t * EPS_t + \varepsilon_t. \tag{5}$$

Here, EPS_t is the EPS for fiscal Year t and EPS_{t3} is the sum of EPS in fiscal Years $t+1$ to $t+3$, both undeflated. Our interest is the coefficient on IS_t * EPS_t, which should be positive if income smoothing strengthens the relation between current and future earnings.

IV. DATA AND MAIN EMPIRICAL RESULTS

We use the 2004 version of Compustat's combined industrial annual data file and choose 1993–2000 as the sample period for the primary test. The period begins with 1993 because 1988 is the first year in which firms are required to report cash flow statements, and we use five observations of ΔDAP and ΔPDI to calculate the income-smoothing measure. Firms in the financial and regulated industries are excluded due to their unique nature of accounting (SIC 4000–4999 and 6000–6999).

Estimation of Discretionary Accruals

For this estimation, we use the data from 1988–2000 and estimate Regression (1) on each of the 650 industry-year cross-sections, after excluding 110 cross-sections that have fewer than 10 observations and winsorizing the regression variables at three standardization deviations each year. Table 1 presents the mean, standard deviation, median, minimum, and maximum of the coefficient estimates and R^2. The coefficients on $1/Assets_{t-1}$ and PPE_t are comparable to those reported in Subramanyam (1996), and the coefficient on $\Delta Sales_t$ is lower than that in Subramanyam due to our additional control for earnings performance. The coefficient on ROA_t has a mean of 0.457, confirming that accruals are associated with firm performance. We calculate a firm's asset-deflated nondiscretionary and discretionary accruals as the fitted values and residuals, respectively.

Income-Smoothing Measure and Data Cleaning

PDI is calculated as net income minus *DAP*, both deflated by the beginning-of-year total assets. A firm-year observation is deleted if its ΔDAP or ΔPDI is missing in the current year or any of the past four years. The income-smoothing measure is calculated for the remaining firm-year observations.

For the primary test, we delete the firm-year observations that have missing data for past, current, and future three years' earnings, operating cash flows, and accruals as well as those for current and future three years' returns. To minimize the effect of outliers, we delete the observations that are in the top or bottom 1 percent of the distributions of the above variables. Even with this effort, extreme outliers are still observed. We further delete the observations whose earnings, operating cash flows, or total accruals in the past, current, or any of the future three years are greater than 10 times or less than -10 times the market equity value, or whose future three years' compound returns are greater than 10 or less than -10. These procedures result in 17,019 observations for the primary test.

Primary Model Test Results

Panel A of Table 2 provides the descriptive statistics of the 17,019 sample observations. The first five rows list the variables in the primary test. *Accruals* and *CFO* will be used in the extended model for a robustness test. The last two rows provide information about the raw income-smoothing measure and *DAP*.

Panel B of Table 2 presents the pairwise correlations between the variables used in Regression (4). The raw income-smoothing measure is negatively associated with past,

TABLE 1
Cross-Sectional Estimation of Discretionary Accruals

The Jones Model, modified by Kothari et al. (2005):

$$Accruals_t = a(1/Assets_{t-1}) + b\Delta Sales_t + cPPE_t + dROA_t + \mu_t$$

Statistics	a	b	c	d	R^2
Mean	0.112	0.013	−0.074	0.457	0.642
Std. Dev.	2.528	0.191	0.129	0.326	0.229
Median	0.068	0.016	−0.077	0.440	0.668
Minimum	−60.167	−3.072	−1.289	−0.810	0.031
Maximum	6.413	1.003	1.819	1.743	1.000

The table presents the summary statistics of the estimated coefficients and R^2 of 650 industry-year regressions from 1988–2000, where industries are classified by the first two digits of the SIC code.

Variable Definitions:

$Accruals_t$ = the total accruals in Fiscal Year *t* obtained by subtracting operating cash flows from net income before extraordinary items and discontinued operations (Compustat Data18), deflated by the beginning-of-year total assets (Compustat Data6);

$Assets_{t-1}$ = the total assets at the beginning of Fiscal Year *t*;

$\Delta Sales_t$ = the change in sales (Compustat Data12) from Fiscal Years $t-1$ to *t*;

PPE_t = the gross property, plant and equipment (Compustat Data7) at the end of Fiscal Year *t*; and

ROA_t = the ratio of net income over the beginning-of-year total assets for Fiscal Year *t*.

current, and future earnings. This indicates that firms with better performance smooth income to a larger degree, consistent with the prediction of the signaling argument discussed in Section II.

Table 3 reports the main test results. First, in Panel A we present the results of Regression (5), the traditional earnings persistence model.[10] As predicted, the coefficient on the interaction between *IS* and *EPS* is significantly positive ($a_3 = 0.703$, t-statistic = 11.24), confirming that income smoothing strengthens earnings persistence.

Second, to compare with previous research using CKSS, in Panel B of Table 3 we present the results of the benchmark CKSS model (Regression (3)). As predicted, both the ERC and FERC are significantly positive. The positive FERC indicates that a significant amount of information about future earnings has been impounded in current stock price. The coefficient on past earnings and future returns are both negative, as predicted.

Panel C of Table 3 reports the results of our primary model. After we include the income-smoothing variable *IS*, the interaction term $IS_t * X_{t3}$ has a significantly positive loading ($b_8 = 0.308$, t-statistic = 4.99), indicating that income smoothing enhances the FERC. The evidence supports the view that income smoothing improves the informativeness of past and current earnings about future earnings. Income smoothing also improves the ERC, evidenced by the significantly positive coefficient on $IS_t * X_t$ ($b_7 = 0.681$, t-statistic = 4.08), consistent with Hunt et al. (2000).

The coefficients on IS_t and $IS_t * X_{t-1}$ (b_5 and b_6) are both significant, confirming their importance as control variables, even though we are primarily interested in the effect of smoothing on FERC and ERC (b_8 and b_7). Although the interaction $IS_t * X_{t3}$ is significantly positive, the coefficient on X_{t3} loses its significance after the inclusion of income smoothing.

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[10] The results are similar if the EPS variables are deflated by the stock price at the beginning of Year *t*.

TABLE 2
Sample Statistics

Panel A: Descriptive Statistics (17,019 Firm-Year Observations during 1993–2000)

Variable	Mean	Std. Dev.	Median	Minimum	Maximum
R_t	0.153	0.688	0.047	−0.949	10.000
X_{t-1}	−0.002	0.185	0.043	−3.480	0.370
X_t	0.015	0.145	0.047	−1.985	0.508
X_{t3}	0.074	0.362	0.125	−3.154	2.238
R_{t3}	0.335	1.162	0.071	−0.998	9.992
ACC_t	−0.070	0.165	−0.038	−2.177	0.609
CFO_t	0.086	0.151	0.078	−1.043	1.246
$Corr(\Delta DAP_t, \Delta PDI_t)$	−0.709	0.418	−0.899	−1.000	1.000
DAP_t	−0.047	0.525	−0.023	−41.540	4.349

Panel B: Pairwise Pearson (Spearman) Correlations above (below) the Diagonal (17,019 observations)

	R_t	X_{t-1}	X_t	X_{t3}	R_{t3}	$Corr\ (\Delta DAP_t, \Delta PDI_t)$
R_t		−0.020	0.188	0.080	−0.115	0.006#
X_{t-1}	0.090		0.466	0.315	0.040	−0.201
X_t	0.401	0.547		0.450	0.040	−0.170
X_{t3}	0.292	0.395	0.532		0.363	−0.138
R_{t3}	−0.069	0.146	0.141	0.539		−0.022
$Corr\ (\Delta DAP_t, \Delta PDI_t)$	−0.058	−0.243	−0.216	−0.188	−0.091	

# Indicates statistically insignificant. The unmarked correlations are statistically significant at 5 percent or lower in a two-tailed test.

Variable Definitions:

R_t = the ex-dividend stock return during Fiscal Year t;

X_{t-1} = the earnings per share (Compustat Data58, adjusted for stock splits and stock dividends) for Fiscal Year $t-1$, deflated by the stock price at the beginning of Fiscal Year t;

X_t = the earnings per share for Fiscal Year t, deflated by the stock price at the beginning of Fiscal Year t;

X_{t3} = the sum of earnings per share for Fiscal Years $t+1$ through $t+3$, deflated by the stock price at the beginning of Fiscal Year t;

R_{t3} = the annually compounded stock return for Fiscal Years $t+1$ through $t+3$;

ACC_t = the total accruals in Fiscal Year t obtained by subtracting operating cash flows from net income before extraordinary items and discontinued operations (Compustat Data18). For the returns regression, different from Accruals in Table 1, this variable is deflated by the market value at the beginning of Fiscal Year t;

CFO_t = the cash flows from operations reported in the cash flow statements (Compustat Data308) for Fiscal Year t, deflated by the market value at the beginning of the year;

$Corr(\Delta DAP_t, \Delta PDI_t)$ = the Pearson correlation between the change in discretionary accruals and the change in pre-managed income; and

DAP_t = the discretionary accruals for Fiscal Year t, deflated by the beginning-of-year total assets.

This suggests that stock price impounds information about future earnings only in the presence of income smoothing.

V. EXTENSION AND ROBUSTNESS TESTS

We have several concerns about the primary model. In this section we report how we extend the model to address these concerns.

TABLE 3
Main Tests

Panel A: Earnings-Persistence Model

$EPS_{t3} =$	a_0	$+ a_1 EPS_t$	$+ a_2 IS_t$	$+ a_3 IS_t * EPS_t + \varepsilon_t$	Adjusted R^2
	0.157	1.034	0.658	0.703	0.278
	(2.74)	(32.06)	(6.56)	(11.24)	

Panel B: Benchmark CKSS Model

$R_t =$	b_0	$+ b_1 X_{t-1}$	$+ b_2 X_t$	$+ b_3 X_{t3}$	$+ b_4 R_{t3} + u_t$	Adjusted R^2
	0.155	−0.535	1.074	0.146	−0.086	0.068
	(28.93)	(−17.07)	(25.04)	(8.46)	(−18.15)	

Panel C: Primary Model

$R_t =$	b_0	$+ b_1 X_{t-1}$	$+ b_2 X_t$	$+ b_3 X_{t3}$	$+ b_4 R_{t3}$	Adjusted R^2
	0.186	−0.296	0.856	−0.002	−0.084	0.072
	(16.30)	(−5.66)	(11.21)	(−0.07)	(−8.88)	

	$+ b_5 IS_t$	$+ b_6 IS_t * X_{t-1}$	$+ b_7 IS_t * X_t$	$+ b_8 IS_t * X_{t3}$	$+ b_9 IS_t * R_{t3} + \varepsilon_t$
	−0.069	−0.296	0.681	0.308	−0.007
	(−3.61)	(−5.66)	(4.08)	(4.99)	(−0.43)

Panel D: Extended Model—Earnings Decomposition

$R_t =$	b_0	$+ b_1 CFO_{t-1}$	$+ b_2 CFO_t$	$+ b_3 CFO_{t3}$	$+ b_4 ACC_{t-1}$	$+ b_5 ACC_t$	$+ b_6 ACC_{t3}$	$+ b_7 R_{t3}$
	0.131	−0.829	1.065	0.064	−0.227	0.657	−0.183	−0.085
	(9.61)	(−8.25)	(10.09)	(2.02)	(−3.93)	(8.77)	(−6.13)	(−9.10)

	$+ b_8 IS_t$	$+ b_9 IS_t * CFO_{t-1}$	$+ b_{10} IS_t * CFO_t$	$+ b_{11} IS_t * CFO_{t3}$
	−0.064	0.023	0.589	0.160
	(−2.76)	(0.13)	(2.96)	(2.81)

	$+ b_{12} IS_t * ACC_{t-1}$	$+ b_{13} IS_t * ACC_t$	$+ b_{14} IS_t * ACC_{t3}$	$+ b_{15} IS_t * R_{t3} + \varepsilon_t$	Adjusted R^2
	−0.687	0.924	0.264	−0.002	0.084
	(−5.02)	(5.61)	(4.79)	(−0.10)	

The number of observations is 17,019.

Variable Definitions:

R_t = the ex-dividend stock return for Fiscal Year *t*;

EPS_t = the earnings per share (Compustat Data58, adjusted for stock splits and stock dividends) for Fiscal Year *t*, undeflated;

EPS_{t3} = the sum of earnings per share for Fiscal Years *t*+1 through *t*+3, undeflated;

X_{t-1} = the earnings per share (Compustat Data58, adjusted for stock splits and stock dividends) for Fiscal Year *t*−1, deflated by the stock price at the beginning of Fiscal Year *t*;

X_t = the earnings per share for Fiscal Year *t*, deflated by the stock price at the beginning of Fiscal Year *t*;

X_{t3} = the sum of earnings per share for Fiscal Years *t*+1 through *t*+3, deflated by the stock price at the beginning of Fiscal Year *t*;

R_{t3} = the annually compounded stock return for Fiscal Years *t*+1 through *t*+3;

IS_t = the reversed fractional ranking of the Pearson correlation between the current year and past four years' change in discretionary accruals and change in pre-managed income;

CFO_{t-1} = the operating cash flows (Compustat Data308) for Fiscal Year *t*–1, deflated by the market value at the beginning of Fiscal Year *t*;

CFO_t = the operating cash flows for Fiscal Year *t*, deflated by the market value at the beginning of Fiscal Year *t*;

CFO_{t3} = the operating cash flows for Fiscal Years *t*+1 through *t*+3, deflated by the market value at the beginning of Fiscal Year *t*;

ACC_{t-1} = the total accruals for Fiscal Year *t*−1, obtained by subtracting operating cash flows (Compustat Data308) from net income before extraordinary items and discontinued operations (Compustat Data18), deflated by the market value at the beginning of Fiscal Year *t*;

ACC_t = the total accruals for Fiscal Year *t*, deflated by the market value at the beginning of Fiscal Year *t*; and

ACC_{t3} = the total accruals for Fiscal Years *t*+1 through *t*+3, deflated by the market value at the beginning of Fiscal Year *t*.

Decomposing Earnings into Cash Flows and Accruals

Although earnings are positively correlated with operating cash flows, predicting cash flows is the main task of equity valuation. Thus, we extend the model to examine whether income smoothing allows more information about future cash flows to be impounded in current stock price.[11] In Regression (6), we decompose earnings into *CFO* and accruals (*ACC*). Our key interest is the interaction between income smoothing and future cash flows ($IS_t * CFO_{t3}$). If income smoothing enhances earnings informativeness about future cash flows, then the coefficient b_{11} should be positive; if income smoothing garbles information, then b_{11} should be negative. Because we are unaware of any theory of or empirical evidence on how income smoothing affects the predictability of future accruals, we have no prediction for the coefficient on $IS_t * ACC_{t3}$.

$$\begin{aligned} R_t = {} & b_0 + b_1 CFO_{t-1} + b_2 CFO_t + b_3 CFO_{t3} + b_4 ACC_{t-1} + b_5 ACC_t + b_6 ACC_{t3} \\ & + b_7 R_{t3} + b_8 IS_t + b_9 IS_t * CFO_{t-1} + b_{10} IS_t * CFO_t + b_{11} IS_t * CFO_{t3} \\ & + b_{12} IS_t * ACC_{t-1} + b_{13} IS_t * ACC_t + b_{14} IS_t * ACC_{t3} \\ & + b_{15} IS_t * R_{t3} + \varepsilon_t. \end{aligned} \tag{6}$$

Panel D of Table 3 reports the test results. The coefficient on $IS_t * CFO_{t3}$ is significantly positive ($b_{11} = 0.160$, t-statistic = 2.81), suggesting that the stock price of firms that engage in more income smoothing impounds more information about their future cash flows. This finding is consistent with our primary results when earnings are used. Note that the coefficient on $IS_t * ACC_{t3}$ is also significantly positive ($b_{14} = 0.264$, t-statistic = 4.79), indicating that stock price also captures more information about future accruals when firms report smoother earnings.

Controlling for Potentially Omitted Correlated Variables

We are concerned that the statistical significance relating to income smoothing may be due to omitted correlated variables. Other factors could make stock price impound more information about future earnings. Omitting these factors would overstate the statistical inference of *IS*. For example, larger firms may make more disclosures for fear of litigation risk (Kasznik and Lev 1995; Johnson et al. 2001). Anticipating future access to the capital markets, firms with higher growth prospects perhaps disclose more forward-looking information to reduce information asymmetry (Frankel et al. 1995). If a firm's future earnings are volatile, then they are more difficult to predict and thus the amount of future earnings information impounded in current stock price is low. In addition, a firm's stock price probably impounds more information about future earnings when there are more private information search activities by analysts and institutional investors.

To address these concerns, we control for firm size, growth, future earnings variability, analyst following, and institutional holdings. Firm size (*Size*) is measured as the market value of common equity at the beginning of Year *t* (Compustat Data199*Data25). Firm growth is proxied by the book-to-market ratio (*BM*) at the beginning of Year *t* (Penman 1996), which is measured as the ratio of book value of common equity (Compustat Data60) over market value of equity. For future earnings variability (*EarnStd*), we use the standard deviation of EPS (Compustat Data58, adjusted for stock splits and stock dividends) for

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[11] Another reason for the earnings decomposition is that smoothing reduces the variance of earnings, and thus may increase the ERC and FERC by construction.

Years $t+1$ to $t+3$, deflated by the stock price at the beginning of Year t. These data requirements reduce the number of observations from 17,019 to 17,011.

Analyst following (*Analysts*) is measured as the average number of analysts' forecasts included in the monthly consensus, compiled by I/B/E/S during Year t. Among the 17,011 observations, 11,879 are covered by I/B/E/S with the mean and median analyst coverage being 7.746 and 4.909, respectively (untabulated). Following Frankel and Li (2004), we set the number of analyst following to zero if a firm-year is not covered by I/B/E/S.

Institutional holdings (*Institution*) are obtained from CDA/Spectrum and measured as the average proportion of shares held by institutional investors at the end of each quarter of Year t. The variable is treated as missing if a firm-year is not covered by CDA/Spectrum. We delete 61 observations for which the institutional holding ratio is greater than 1 (data error). For the remaining 16,950 observations, 13,954 are covered by CDA/Spectrum with the mean and median institutional holding ratio being 0.367 and 0.356, respectively (untabulated).

The new control variables are converted into fractional rankings within their industry-year before they enter the regression.[12] We add new control variables to the primary model one at a time, referred to as Z_t in Regression (7). The control is exercised through the interaction $Z_t * X_{t3}$. Variable Z_t is included because omitting it would make the interpretation of the coefficient on $Z_t * X_{t3}$ problematic if Z_t directly affects returns.

$$\begin{aligned} R_t = {} & b_0 + b_1 X_{t-1} + b_2 X_t + b_3 X_{t3} + b_4 R_{t3} + b_5 IS_t + b_6 IS_t * X_{t-1} \\ & + b_7 IS_t * X_t + b_8 IS_t * X_{t3} + b_9 IS_t * R_{t3} + b_{10} Z_t + b_{11} Z_t * X_{t3} + \varepsilon_t. \end{aligned} \tag{7}$$

Panel A of Table 4 reports the estimation results. Throughout the individual models, the coefficients on $IS_t * X_{t3}$ remain significantly positive, supporting our previous conclusion that income smoothing improves earnings informativeness. In addition, the coefficients on the interactions between X_{t3} and firm size, growth, analyst following, and institutional holdings are positive, and the coefficient on the interaction between X_{t3} and future earnings variability is negative.[13] These results confirm that the information environment is richer for large high-growth firms and firms with high analyst coverage and institutional holdings, and that stock price contains less information about future earnings when these earnings are more difficult to predict.

Panel B of Table 4 reports the test results with all five new controls in place, using the observations that have institutional holdings data. The coefficient on $IS_t * X_{t3}$ is weakly significantly positive in a two-tailed test ($b_8 = 0.118$, t-statistic $= 1.83$). The coefficients on the interactive terms for size, growth, and future earnings variability are similar to those in Panel A. In the presence of these controls, the level of analyst coverage is associated with lower FERC ($b_{17} = -0.149$, t-statistic $= -1.91$),[14] and institutional holdings are unrelated to FERC ($b_{19} = -0.06$, t-statistic $= -0.66$).

Finally, in Panel C of Table 4, we create two dummy variables so that we can use the information about analyst following and institutional holdings, when the information is

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[12] In case of ties, the lowest corresponding ranks are assigned. For observations that are not covered by I/B/E/S, the analyst-coverage rankings are set to zero.

[13] Alternatively, we estimate regressions in which each of the new control variables interacts with all existing independent variables and find similar results. To save space, we report our results for Regression (7) only.

[14] For this sample of 13,954 observations, if we include only "Analysts" as Z_t, the coefficient on $Z_t * X_{t3}$ is 0.369 with a t-statistic of 7.14, both of which are very similar to the results in Panel A for the full sample of 17,011 observations. This confirms that it is the additional controls, and not the change in sample, that reduces the effect of analyst coverage in the regression model in Panel B.

TABLE 4
Robustness Tests
Controlling for Potentially Omitted Correlated Variables

Panel A: Adding a Single New Control Variable Z_t (variable definitions are provided at the end of the table)

$$R_t = b_0 + b_1X_{t-1} + b_2X_t + b_3X_{t3} + b_4R_{t3} + b_5IS_t + b_6IS_t * X_{t-1}$$
$$+ b_7IS_t * X_t + b_8IS_t * X_{t3} + b_9IS_t * R_{t3} + b_{10}Z_t + b_{11}Z_t * X_{t3} + \varepsilon_t$$

Z_t =	$Size_t$	BM_t	$EarnStd_t$	$Analysts_t$	$Institution_t$
Intercept	0.228	0.499	−0.092	0.163	0.155
	(16.13)	(36.88)	(−5.71)	(12.71)	(8.54)
X_{t-1}	−0.277	−0.239	−0.267	−0.304	−0.221
	(−5.31)	(−4.77)	(−5.21)	(−5.83)	(−3.59)
X_t	0.889	0.924	0.799	0.856	0.890
	(11.66)	(12.64)	(10.63)	(11.21)	(9.74)
X_{t3}	−0.190	−0.162	0.866	−0.108	−0.239
	(−5.03)	(−4.22)	(14.49)	(−3.05)	(−4.73)
R_{t3}	−0.087	−0.064	−0.101	−0.087	−0.095
	(−9.27)	(−7.07)	(−10.88)	(−9.21)	(−9.04)
IS_t	−0.074	−0.009	−0.054	−0.080	−0.085
	(−3.82)	(−0.50)	(−2.84)	(−4.16)	(−3.98)
$IS_t * X_{t-1}$	−0.636	−0.514	−0.631	−0.659	−0.985
	(−5.12)	(−4.31)	(−5.16)	(−5.30)	(−6.65)
$IS_t * X_t$	0.668	0.541	0.594	0.690	0.753
	(4.02)	(3.38)	(3.63)	(4.14)	(3.87)
$IS_t * X_{t3}$	0.291	0.199	0.265	0.298	0.267
	(4.74)	(3.34)	(4.37)	(4.87)	(3.82)
$IS_t * R_{t3}$	−0.011	−0.008	−0.009	−0.008	−0.001
	(−0.63)	(−0.53)	(−0.54)	(−0.48)	(−0.08)
Z_t	−0.118	−0.674	0.438	0.039	0.052
	(−6.08)	(−39.42)	(22.91)	(2.49)	(2.07)
$Z_t * X_{t3}$	0.630	0.265	−0.973	0.382	0.599
	(10.77)	(6.26)	(−16.30)	(8.32)	(8.25)
Adjusted R^2	0.079	0.150	0.105	0.077	0.085
Observations	17,011	17,011	17,011	17,011	13,954

Panel B: Full Model (13,954 observations that have institutional holdings data)

$R_t =$	b_0	$+ b_1X_{t-1}$	$+ b_2X_t$	$+ b_3X_{t3}$	$+ b_4R_{t3}$	Adjusted R^2
	0.316	−0.029	0.964	0.555	−0.089	0.252
	(12.65)	(−0.53)	(11.66)	(7.02)	(−9.37)	

$+ b_5IS_t$	$+ b_6IS_t * X_{t-1}$	$+ b_7IS_t * X_t$	$+ b_8IS_t * X_{t3}$	$+ b_9IS_t * R_{t3}$
0.052	−0.720	0.351	0.118	−0.015
(2.67)	(−5.37)	(1.99)	(1.83)	(−0.90)

$+ b_{10}Size_t$	$+ b_{11}Size_t * X_{t3}$	$+ b_{12}BM_t$	$+ b_{13}BM_t * X_{t3}$	$+ b_{14}EarnStd_t$	$+ b_{15}EarnStd_t * X_{t3}$
−0.551	0.621	−0.965	0.474	0.639	−1.012
(−15.52)	(5.82)	(−48.76)	(9.81)	(30.27)	(−15.80)

$+ b_{16}Analysts_t$	$+ b_{17}Analysts_t * X_{t3}$	$+ b_{18}Insitution_t$	$+ b_{19}Insitution_t * X_{t3} + \varepsilon_t$
0.164	−0.149	0.229	−0.060
(5.87)	(−1.91)	(7.48)	(−0.66)

(continued on next page)

TABLE 4 (Continued)

Panel C: Full Model (16,950 observations)

$$\begin{array}{llllllr} R_t = & b_0 & + b_1X_{t-1} & + b_2X_t & + b_3X_{t3} & + b_4R_{t3} & \text{Adjusted } R^2 \\ & 0.323 & -0.135 & 0.920 & 0.560 & -0.082 & 0.232 \\ & (13.31) & (-2.84) & (13.16) & (8.91) & (-9.50) & \end{array}$$

$$\begin{array}{lllll} + b_5IS_t & + b_6S_t * X_{t-1} & + b_7IS_t * X_t & + b_8IS_t * X_{t3} & + b_9IS_t * R_{t3} \\ 0.051 & -0.335 & 0.353 & 0.129 & -0.018 \\ (2.85) & (-2.95) & (2.32) & (2.26) & (-1.19) \end{array}$$

$$\begin{array}{llllll} + b_{10}Size_t & + b_{11}Size_t * X_{t3} & + b_{12}BM_t & + b_{13}BM_t * X_{t3} & + b_{14}EarnStd_t & + b_{15}EarnStd_t * X_{t3} \\ -0.491 & 0.532 & -0.894 & 0.376 & 0.619 & -0.978 \\ (-15.45) & (5.92) & (-49.76) & (8.95) & (32.00) & (-16.75) \end{array}$$

$$\begin{array}{ll} + b_{16}(Analysts_t * Dumcover_t) & + b_{17}(Analysts_t * Dumcover_t) * X_{t3} \\ 0.191 & -0.148 \\ (5.14) & (-2.15) \end{array}$$

$$\begin{array}{ll} + b_{18}(Institution_t * Dumhold_t) & + b_{19}(Insitution_t * Dumhold_t) * X_{t3} \\ 0.165 & 0.018 \\ (5.81) & (0.32) \end{array}$$

$$\begin{array}{lll} + b_{20}Dumcover_t & + b_{21}Dumhold_t & + \varepsilon_t \\ -0.002 & -0.040 & \\ (-0.07) & (-2.06) & \end{array}$$

The first four columns of Panel A use 17,011 observations. The number of observations is less than that in Table 3 because of missing data for firm size, B/M, and earnings variability (8 observations).

The last column of Panel A and Panel B use 13,954 observations that have institutional holdings data.

Panel C uses 16,950 observations. The number of observations is 61 less than 17,011 because of data error in institutional holdings. The estimation uses the I/B/E/S and CDA/Spectrum information when it is available, and uses the full sample to estimate the coefficients unrelated to analyst following and institutional holdings.

Variable Definitions:

$Size_t$ = the within industry-year fractional ranking (between 0 and 1) of a firm's market value (Compustat Data199*Data25) at the beginning of Fiscal Year t;

BM_t = the within industry-year fractional ranking (between 0 and 1) of a firm's book-to-market ratio (Compustat Data60/(Data199*Data25)) at the beginning of Fiscal Year t;

$EarnStd_t$ = the within industry-year fractional ranking (between 0 and 1) of a firm's standard deviation of earnings per share (Compustat Data58, adjusted for stock splits and stock dividends) for Fiscal Years $t+1$ to $t+3$, deflated by the stock price at the beginning of Fiscal Year t;

$Analysts_t$ = the within industry-year fractional ranking (between 0 and 1) of a firm's average number of analyst forecasts included in the monthly consensus, compiled by I/B/E/S during the fiscal year. If a firm-year is not covered by I/B/E/S, the number of analyst following is set to 0;

$Institution_t$ = the within industry-year fractional ranking (between 0 and 1) of a firm's average proportion of shares held by institutional investors at the end of each quarter of Fiscal Year t, obtained from the CDA/Spectrum database. If a firm-year is not covered by CDA/Spectrum, this variable is treated as missing in Panel B and the last column of Panel A, and is set to 0 in Panel C;

$Dumcover_t$ = 1 if a firm-year is covered by I/B/E/S, and 0 otherwise; and

$Dumhold_t$ = 1 if a firm-year is covered by the CDA/Spectrum institutional holdings database. and 0 otherwise.

See Table 3 for the definitions of other variables.

available, to estimate the coefficients relating to these two controls, and we can use the full sample to estimate other coefficients. $Dumcover_t$ takes the value of 1 for the firm-years covered by I/B/E/S, and 0 otherwise. $Dumhold_t$ takes the value of 1 for those covered by CDA/Spectrum, and 0 otherwise. The results for the control variables are similar to those in Panel B. The reported coefficient on $IS_t * X_{t3}$ is increased to 0.129, significantly positive (t-statistic 2.26), confirming that income smoothing improves earnings informativeness.

Profit versus Loss Firms

Prior research has demonstrated that profits are more value-relevant than losses because (1) losses are more transitory (Basu 1997) and (2) the values of loss firms are bounded below by the liquidation option (Hayn 1995). Among the 17,019 observations used for the main test, 4,391 observations (25.8 percent) have current losses. In Regression (8), we add a dummy variable for current year losses and its interaction with X_t and X_{t3}.

$$\begin{aligned} R_t = {} & b_0 + b_1X_{t-1} + b_2X_t + b_3X_{t3} + b_4R_{t3} + b_5IS_t + b_6IS_t * X_{t-1} \\ & + b_7IS_t * X_t + b_8IS_t * X_{t3} + b_9IS_t * R_{t3} + b_{10}Loss_t + b_{11}Loss_t * X_t \\ & + b_{12}Loss_t * X_{t3} + \varepsilon_t. \end{aligned} \tag{8}$$

Table 5 shows that, with this control, income smoothing does not change the ERC but enhances the FERC (coefficient 0.172 and t-statistic 2.84), indicating that the stock price of higher income smoothing firms impounds more information about future earnings than that of lower income smoothing firms. For loss firms, both the ERC and FERC attenuate. The significantly negative coefficient on $Loss_t * X_t$ indicates that the ERC for loss firms is lower than that for profit firms, consistent with prior research. The significantly negative coefficient on $Loss_t * X_{t3}$ suggests that the stock price of loss firms reflects less information about their future earnings than that of profit firms.

Fama-MacBeth Regressions

In estimating cross-sectional regressions, the potential positive cross-sectional correlations of the residuals are a valid concern. If they exist, then our inferences are overstated. To address this concern, we extend the sample period to pre-1988 data (before the advent of the cash flow statements) to obtain a large number of cross-sections for the Fama-MacBeth analysis. For pre-1988, we use the "balance sheet" approach to estimate total accruals.[15]

TABLE 5
Robustness Tests
Profit versus Loss Firms

$R_t =$	b_0	$+ b_1X_{t-1}$	$+ b_2X_t$	$+ b_3X_{t3}$	$+ b_4R_{t3}$	$+ b_5IS_t$	$+ b_6IS_t * X_{t-1}$	Adjusted R^2
	−0.000	−0.260	3.608	0.116	−0.079	−0.053	−0.415	0.123
	(−0.02)	(−5.12)	(26.86)	(3.11)	(−8.58)	(−2.78)	(−3.42)	

$+ b_7IS_t * X_t$	$+ b_8IS_t * X_{t3}$	$+ b_9IS_t * R_{t3}$
0.082	0.172	−0.010
(0.50)	(2.84)	(−0.63)

$+ b_{10}Loss_t$	$+ b_{11}Loss_t * X_t$	$+ b_{12}Loss_t * X_{t3}$	$+ \varepsilon_t$
−0.004	−3.259	−0.334	
(−0.25)	(−25.67)	(−10.28)	

The number of observations is 17,019.
$Loss_t = 1$ if a firm reports negative earnings for Fiscal Year t and 0 otherwise.
See Table 3 for the definitions for other variables.

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[15] The operating cash flows are measured as net income before extraordinary items minus the increase in noncash current assets, plus the increase in current liabilities (excluding the short-term portion of long-term debt) and plus depreciation expense (Collins and Hribar 2002).

The results are reported in Table 6. The left columns of the table report the results of the primary model (4), and the right columns report the results of the extended model (6). The table reports the means, medians, and t-statistics of the three key coefficients in the 21 annual regressions from 1980 to 2000. The mean coefficient on $IS_t * X_t$ is 1.038, significantly positive (Fama-MacBeth t-statistic 8.94). The mean coefficient on $IS_t * X_{t3}$ is 0.142, also significantly positive (Fama-MacBeth t-statistic 2.61). The right columns of the table report the earnings-decomposition regressions. The mean coefficients on $IS_t * CFO_t$ and on $IS_t * CFO_{t3}$ are 1.045 and 0.133, respectively, both significantly positive (Fama-MacBeth t-statistics 8.93 and 2.59, respectively). Overall, the results confirm our primary test results.

In summary, the robustness-test results are consistent and support the conclusion that income smoothing improves earnings informativeness about future earnings and cash flows.

VI. CONCLUSION

We use a new approach to investigate whether income smoothing garbles accounting earnings information or improves the informativeness of firms' current and past earnings about their future earnings and cash flows. We measure income smoothing as the negative correlation of a firm's change in discretionary accruals with its change in pre-managed income. A more negative correlation indicates more income smoothing.

Using the method of Collins et al. (1994), we find that a higher-smoothing firm's future earnings are impounded in its current stock price to a larger extent than that of a lower-smoothing firm. Such results are robust after we decompose earnings into operating cash

TABLE 6
Robustness Tests
Fama-MacBeth Regressions

Primary Model:

$$\begin{aligned} R_t = {} & b_0 + b_1 X_{t-1} + b_2 X_t + b_3 X_{t3} + b_4 R_{t3} + b_5 IS_t + b_6 IS_t * X_{t-1} \\ & + b_7 IS_t * X_t + b_8 IS_t * X_{t3} + b_9 IS_t * R_{t3} + \varepsilon_t \end{aligned}$$

Extended Model:

$$\begin{aligned} R_t = {} & b_0 + b_1 CFO_{t-1} + b_2 CFO_t + b_3 CFO_{t3} + b_4 ACC_{t-1} + b_5 ACC_t + b_6 ACC_{t3} + b_7 R_{t3} \\ & + b_8 IS_t + b_9 IS_t * CFO_{t-1} + b_{10} IS_t * CFO_t + b_{11} IS_t * CFO_{t3} \\ & + b_{12} IS_t * ACC_{t-1} + b_{13} IS_t * ACC_t + b_{14} IS_t * ACC_{t3} + b_{15} IS_t * R_{t3} + \varepsilon_t \end{aligned}$$

Time-Series	Primary Model			Extended Model		
Statistics	X_t	$IS_t * X_t$	$IS_t * X_{t3}$	CFO_t	$IS_t * CFO_t$	$IS_t * CFO_{t3}$
Mean	0.755	1.038	0.142	0.824	1.045	0.133
Median	0.741	1.033	0.170	0.890	0.969	0.166
Fama-MacBeth t-statistic	13.94	8.94	2.61	12.12	8.93	2.59

The models are each estimated on 21 industry-year cross-sections during 1980–2000. The Fama-MacBeth approach treats the coefficients from the annual regressions as i.i.d.

For firm-years post-1988, operating cash flows are obtained from Compustat Data308.

For firm-years pre-1988, operating cash flows are calculated using the balance-sheet approach (Collins and Hribar 2002).

See Table 3 for variable definitions.

flows and accruals; separate loss firms from profit firms; and control for firm size, growth, future earnings variability, private information search activities, and potential cross-sectional correlations. Thus, we document empirically that an important effect of managers' use of financial reporting discretion is to reveal more information about firms' future earnings and cash flows. Our work contributes to the literature by shedding new light on this information-versus-garbling debate.

Our results are subject to two caveats. First, the interpretation of our results critically relies upon the assumption of market efficiency. In the presence of mispricing, our results are subject to reinterpretation. Second, despite all our attempts to ensure that measurement error in the income-smoothing measure is not driving the results, we cannot rule out measurement error as an alternative explanation for the results.

Our paper presents the first empirical evidence that stock prices impound more information about future earnings when firms smooth their reported income. Perhaps more important than its results, the paper presents a new approach to studying the effects of earnings management. The informativeness methodology used here to study income smoothing can be applied to other types of earnings management and thus represents a promising area for future research.

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THE ACCOUNTING REVIEW
Vol. 81, No. 1
2006
pp. 271–279

A Practical Model of Earnings Measurement

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I. INTRODUCTION[1]

A basic tenet of financial accounting posits that valuation/measurements rely on either a balance sheet approach or an income statement approach. In the first case, the (accounting) rules determine the carrying values in the balance sheet, and changes in these lead to measurements of revenues and expenses. In the second case, the rules determine directly the revenues and expenses, and these in turn allow for an update of the various balance sheet values. GAAP, of course, on the whole rests on the balance sheet approach. In particular, GAAP does not accept generic Deferred Expenses or Losses as assets because the valuation of an asset generally requires that there are some underlying property rights (at least implicitly) in place. And any historical cost valuation of property rights cannot be tested against fair values unless they are reasonably specific and identifiable. Deferred Expenses/Losses as assets, on the other hand, arise because certain expenditures have yet to be expensed and these need not represent property rights. With an income statement approach, the asset-balance therefore depends on how one "interprets" a history of transactions. As a matter of construct, such assets do not generally have to be tied to notions of fair values.

There might well be good reasons why GAAP relies on a balance sheet approach. Auditing makes the most sense if it focuses on the valuation of specific and identifiable assets/liabilities rather than the evaluation of histories of transactions. In addition, investors feel comfortable with the labels Assets and Liabilities if they are concrete and identifiable. But there are also disadvantages associated with the balance sheet approach. The main one pertains to the proliferation of special charges that are necessary to prevent the overvaluation of assets relative to estimates of their fair values. Nonrecurring charges in the form of realized gains/losses—which cannot generally be deferred—also show up with some frequency in the income statement. These aspects cause problems because investors like to have a natural starting point in the income statement when they try to forecast subsequent periods' sustainable (or permanent) earnings. Nonrecurring items, of whatever kind, make

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[1] This paper synthesizes ideas developed in a much longer paper, "A Practical Alternative to GAAP Earnings."

This article is based on a Presidential Address at the AAA's annual convention in Orlando, Florida, August 2004. I thank the Research Committee for nominating me, as well as my many friends and colleagues who have stimulated me to engage in traditional accounting theory research, vintage ca 1960s.

Editor's note: The Executive Committee of the American Accounting Association has recommended the commission of a series of Presidential Research Lectures, to be delivered at annual meetings of the Association. To encourage broad dissemination, the Committee has requested that *The Accounting Review* publish this lecture given at the 2004 American Accounting Association Annual Meeting in Orlando, FL.

it difficult for investors to identify a "bottom line" that roughly corresponds to a firm's sustainable earnings.

This paper describes an accounting intended to embody the spirit of sustainable earnings measurement. An income statement approach is thus maintained: Aside from cash and cash-equivalent items in the balance sheet, all other assets should be viewed as generic deferred expenses. Thus, the accounting differs from GAAP because the balance sheet items, aside from cash and approximately equivalent items, do not align with property rights. In no way does the accounting assign historical or fair values to specific balance sheet items such as "Inventories" or "PPE." Instead there are accounting rules that determine periodic expenses such that net income evolves relatively smoothly over time. In some ways, therefore, the proposed accounting is rather unusual and quite different from GAAP. But the accounting involves no divorced-from-reality ideas that would have suggested that the exercise is merely "academic" with a negative connotation. In fact, the accounting fits the standard debit/credit framework and it specifies rules on how to account for all the usual kinds of transactions.

As this accounting model is developed, it will be seen that its core concerns the nature of accruals. Accruals arise because of matching, and the total expense incurred will indeed differ from the corresponding cash outflow component of net income. Expensing—as opposed to total cash expenditures incurred—can either be in the form of a strict period expense (like S&A) or it can be matched to sales (like CGS in GAAP), or both. The latter involves some particularly subtle ingredients, which much of my discussion will deal with.

The outline of the paper is as follows. To get a feel for what the model tries to accomplish, Section II enumerates some aspects inherent in GAAP that the model eschews. Section III overviews the characteristics built into the model. Section IV spells out the general approach, and Section V spells out the sequence of steps one has to go through to implement the model. Section VI summarizes what I view as the model's advantages.

II. PROBLEMS INHERENT IN GAAP TO BE AVOIDED

To motivate the accounting, I will first point out in what ways GAAP is dysfunctional from the perspective of a (potential) equity investor who assesses a firm's income statement. As these negative comments bear on the income statement, I do not want to argue that GAAP, per se, is doing a poor job. GAAP, as noted, relies on a balance sheet approach (for plausible reasons) and thus it makes little sense to criticize GAAP if one postulates a different approach, namely, an income statement approach. That said, it still helps to recognize some of GAAP's problems when the accounting targets a "bottom line" in the income statement. This will help to establish the "flavor" of an alternative model.

Below follows a list of characteristic associated with GAAP that will *not* be part of the proposed accounting.

(1) *Nonrecurring or special charges*. GAAP tends to produce a large number of potential items falling into this category. To mention a few, consider restructuring charges and other write-downs, discontinued operations, effects due to changes in accounting principles, and gains/losses due to dispositions of noncurrent operational assets. Some of these items are relatively subjective, such as restructuring charges, and the related balance sheets values tend to be at least somewhat discretionary as long as they are sufficiently conservative. Investors often consider these items as a "nuisance," in sharp contrast to regular recurring items such as CGS, S&A, and R&D. Earnings measures such as "Pro-Forma Earnings" and "Street Earnings" illustrate how investors try to cope to identify a much-desired, but all

too often elusive, measure of a firm's permanent earnings. To make matters even worse, past nonrecurring items influence subsequent recurring items (e.g., a write-down of inventory reduces subsequent CGS).

(2) *Inconsistent and arbitrary capitalization rules.* A well-known critique of GAAP concerns R&D accounting. Such expenditures are expensed as incurred, in contrast to regular capital expenditures, even though both types of expenditures benefit the future as opposed to the current period. Another idiosyncratic inconsistency pertains to the accounting for purchased R&D as compared to purchased goodwill. A less apparent issue pertains to overhead expenditures allocated to manufactured inventories. These allocations build in arbitrary aspects without a conceptual foundation.[2] Also, aggressive capitalization increases chances of a subsequent write-down.

(3) *Arbitrary complexity inherent in certain standards.* The accounting for pensions and taxes serve as illustrations of arcane and difficult to comprehend standards. The extent to which all the myriad details are necessary to have a useful accounting is not apparent, to put it mildly. Part of the problem relates to the fact that the underlying accounting rules rely on a mixture of a balance sheet and income statement focuses.

(4) *Ambiguous concept of Other Comprehensive Income.* A proliferation of nonrecurring items inevitably leads to questions about the scope of Other Comprehensive Income. FASB has struggled with this issue, but it has never provided a clean concept. The question as to whether Other Comprehensive Income is too narrow or broad defies an answer unless one conceptualizes Net Income itself.

One can readily identify a host of other problems inherent in GAAP, such as the accounting for compensation options and the lack of a "clean" concept of owners' equity (a common shareholders perspective versus GAAP's so-called entity perspective). The proposed accounting does indeed suggest how one should deal with these issues in order to be consistent with the broader framework, but space precludes a more extensive discussion. The points made in this section merely underscore the more obvious problems associated with a "balance-sheet" oriented (or "mixed") approach as compared to one that focuses squarely on the measurement of income.

III. AN OVERVIEW OF THE ACCOUNTING

To appreciate the description of an unfamiliar accounting model always poses a challenge for the reader unless he/she at all points has a sense of the bigger picture. The development accordingly proceeds in stages from broad ideas to implementation aspects, with some repetitions to reinforce the model's key attributes. This section provides the broad overview, and the next one concerns how certain balance sheet items arise as "plugs." The third stage delineates in five steps the specifics to ensure a complete accounting treatment for all reasonable transactions one may encounter. With these ideas in place it becomes reasonably straightforward to visualize the workings of the model in its generality.

As to an overview of the model's key characteristics, the following should be kept in mind:

- The accounting initially picks up only on "actual" transactions that involve cash and their approximate equivalence. That is, prior to the introduction of any accruals the core of the accounting rests on what I will refer to as "modified cash accounting." The word "modified" is intended to capture the idea that it makes sense to extend "pure" cash accounting because some assets and liabilities are sufficiently similar to cash or the negative thereof (e.g., marketable securities, bank loans, accounts payable). Assets/liabilities sufficiently similar to cash should judgmentally aggregate with cash with no, or only minor, loss of information.

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[2] For example, one can reasonably argue that R&D should be allocated to inventories, no less than depreciation. Of course, GAAP does not permit such accounting.

- The balance sheet distinguishes between cash and approximate cash-equivalent assets/liabilities as opposed to other assets/liabilities. The former can be thought of as financial, whereas the latter are operating in nature. The Income Statement maintains a similar dichotomy, i.e., on the one hand, income due to financial activities and, on the other, income due to operating activities.
- Operating activities satisfy the clean surplus relation (CSR), but financial activities need not. Thus, Other Comprehensive Income can include only realized and nonrealized gains/losses due to financial activities.
- There will be no recognized gains/losses or other nonrecurring items associated with operating activities. Whether realized or not per GAAP, the accounting here effectively spreads operating gains/losses over many periods.
- Expenditures incurred due to operating activities will be capitalized and treated as "deferred expenses" whenever they benefit subsequent periods.
- The proto-type model capitalizes all operating expenditures by debiting a master-account, which thus pools all such expenditures.
- Deferred expenses are passed on to the income statement by matching expenses to revenues, akin to cost of goods sold.

All of these points require elaboration, of course. I start out in the next section by laying out a framework that identifies and accounts for operating activities.

IV. FRAMING THE ACCOUNTING FOR OPERATING ACTIVITIES

The accounting splits the balance sheet into two classes of assets, net of liabilities. The net of these two classes added thus equals common shareholders' equity.

The first class is relatively straightforward, and I will refer to it as the firm's financial assets/liabilities. It consists of those assets/liabilities that generally approximately their cash equivalent values. These will be labeled *CCE*, where one reads *CCE* as Cash and Cash Equivalent assets and liabilities. One can think of *CCE* as being equal to shareholders' equity if the firm uses what is commonly referred as "modified cash accounting": Only the unambiguous, "hard," assets/liabilities thus show up on the balance sheet if one uses "modified cash accounting." That is, there are now no "subjective" accruals or estimates. One can reasonably claim that for these kinds of assets/liabilities, GAAP works as it should. There are few, if any, controversies associated with the accounting for cash and approximate cash-equivalent assets and liabilities.

The second class of assets/liabilities will be referred to as *NOA*, where *NOA* stands for net operating assets. For this class we put aside GAAP, and most of the subsequent discussion pertains to their accounting. In principle, though, the balance in *NOA* represents and subsumes the values of operating assets/liabilities such as inventories, prepaid and accrued expenses, property, plant and equipment unamortized, R&D expenditures, purchased goodwill, postretirement obligations, etc. How to account for these kinds of assets/liabilities poses considerable conceptual and practical problems and GAAP tends to approach the various assets/liabilities on a piecemeal basis without relying on any apparent unifying broad valuation principles. The approach here will be quite different.

As noted, the two-class approach implies that:

$$CCE_t + NOA_t = CSE_t$$

where CSE_t denotes common shareholders equity. The accounting maintains a common shareholders' perspective; any preferred stock will be part of CCE_t.

I now come to a central aspect of the accounting for operating activities. The balance sheet account NOA_t evolves over time according to the T-account relation:

$$NOA_t = NOA_{t-1} + Expenditures_t - Expenses_t. \tag{1}$$

To appreciate its workings, first note that NOA_{t-1} is a given because of the accounting cycle. Second, the expenditures for period t can be *inferred* from the following data: the change in the balances in CCE (i.e., ΔCCE_t), the net distribution of wealth to the common shareholders, the cash (equivalent) receipts from customers, and the cash (equivalent) income items due to financial activities. I will refer to these expenditures as the Sales-Sustaining Expenditures, SSE_t for short.

A firm obviously incurs all sorts of operating expenditures to generate sales in the current and future periods, and at this point we need not identify the various kinds. Hence, we can rewrite Equation (1) as:

$$NOA_t = NOA_{t-1} + SSE_t - Expense_t. \tag{2}$$

Third, an explicit accounting *rule* determines the operating expense for any given period. With such a rule in place one can indeed claim that the approach focuses on income measurement as opposed to the direct valuation of an asset (or assets) included in the balance sheet. Specifically, NOA_t—the ending balance of net operating assets—derives as a so-called "plug."

V. STEPS THAT IMPLEMENT THE ACCOUNTING

This section spells out the five steps one follows to produce the financial statements. I view all the steps as concrete and impenetrable as described. Of course, as is always the case in accounting, there may be "judgment" issues, e.g., the extent to which certain assets/liabilities should be part of CCE_t. There will also be certain judgment issues related to the rule that determines the operating expense. But these matters should not overshadow that the proposed accounting is no more complicated, or opaque, than GAAP.

The steps are as follows:

1. Identify all assets/liabilities that approximate cash, i.e., their book values should approximate their market values. One naturally includes cash, marketable securities, high-quality notes receivable, accounts payable, loans and bonds payable, and current taxes legally due various governmental entities. The inclusion of accounts receivable depends on their quality, i.e., they should be included only if their face value approximates their market value. Having so identified the qualifying assets/liabilities, one can now derive the (approximate) cash (and cash equivalent) net worth at each cash date. As in the previous section, let CCE_t represent this "cash and approximate cash equivalent net worth" at date t.
2. Derive the comprehensive cash earnings on the basis of step 1 and capital transactions. Hence one obtains Comprehensive Earnings under Modified Cash Accounting, or $CE - MCA_t$:

Algebraically, elementary accounting implies that:

$$CE - MCA_t = \Delta CCE_t + Net\ Dividends_t$$

where *Net Dividends* refers to all transactions with common shareholders (cash dividends, issuance of stock, and treasury stock transactions). Given the concepts inherent in our modified cash accounting the net dividends term should be measured by the transactions' (fair) market values (this aspect will be relevant in cases of (a) shares exchanged for non-cash assets and (b) options exercised).

3. Prepare an income statement on the basis of modified cash accounting.
Whereas the previous step provided the "bottom line," comprehensive cash earnings ($CE - MCA_t$), the next step identifies the major line items in this income statement. To do so we dichotomize between operating versus financial activities. Financial activities relate to marketable securities (and similar items) and financial obligations. In turn, such assets/liabilities typically imply interest revenues/expenses and basically unpredictable unrealized/realized gains and losses due to changes in market values. Within a context of modified cash accounting one can reasonably presume that the treatment of such transactions poses no problems. We further assume that one can identify and measure the cash equivalent inflows due to transactions with customers.

One obtains the following income statement on the basis of modified cash accounting.

Cash Collected from Customers	XXX
Sales Sustaining Expenditures	<XXX>
Operating Cash Earnings	XXX
Financial Revenues/Expenses, Including Unpredictable Financial Items	XXX
(Comprehensive) Cash Earnings	XXX

Notice that the Sales Sustaining Expenditures, or SSE_t can be *inferred from* (a) the "bottom line," (b) the "top line," and (c) and the financial items.

Without going into the details, it is readily seen that the above framework poses few implementation problems, subject to the usual caveat that one has to exercise "judgments." For example, financial items can be shown net of taxes so that operating cash earnings is also implicitly net of taxes. Because the accounting is one of modified cash accounting, this is done without the use of a Deferred Taxes account (in sharp contrast to GAAP). As another example, one can adjust the above framework to separate any transactions related to investments accounted for under the equity method.

4. Capitalize and expense the sales sustaining expenditures: The general framework. The basic approach to introduce accruals replaces only one item in the previous income statement, namely, Sales Sustaining Expenditures. In terms of the balance sheet, all items will be the same as in step 2, except that we introduce a "master-account" of unamortized Sales Sustaining Expenditures. Thus, there is full consistency as to the balance sheets and the income statements. As previously indicated, I will refer to the unamortized sales sustaining expenditures as *NOA*, or net operating assets. The underlying T-account expressed by Equation (2) applies and where

we now underscore that the expense is indeed an operating expense:

$$NOA_t = NOA_{t-1} + SSE_t - OpExpense_t$$

In this framework SSE_t is as derived in the previous step, $OpExpense_t$ is determined by an "accounting principle," NOA_{t-1} is a "given" and NOA_t the residual "plug." One sees that, indeed, $\Delta NOA_t = SSE_t - OpExpense_t$ defines the net periodic accrual, all of which pertain to operating activities. This accrual subsumes the totality of operating activities, such as compensation/wages, services provided by vendors, R&D, capital expenditures net of dispositions, acquisitions, cost-of-goods sold, etc. All of these are on an accrual basis, though, of course, at this point there are no line items for the various types of expenses.

We now come to the initial stage of describing the rule that determines *OpExpense* for each period. The basic idea is that the expense matches with the cash equivalent sales revenue for the period, SR_t, Specifically, consider the rule:

$$(*) \qquad OpExpense_t = \alpha \cdot \frac{NOA_{t-1}}{SR_{t-1}} \cdot SR_t,$$

where α is a constant. The determination of α will be discussed in the next step. Here I simply state that one can think of α as directly affecting the extent to which the accounting is conservative. To appreciate this point, note that in a steady state $SR_t = SR_{t-1}$ and $SSE_t = OpExpense_t$ so that $NOA_t = NOA_{t-1} = \alpha^{-1} \cdot SSE_t$. A large α thus implies a small unchanging balance noa_t. In this sense a large α leads to conservative accounting from a balance sheet perspective.

A careful reader might reasonably wonder about the origins of the rule (*). What ideas lead to this rule? The answer to this question is actually fairly straightforward if one recognizes what an accounting ought to look like under "perfect conditions." Consider the following result.

Proposition:[3] Any two of the following three statements implies the third:

$$\text{(i)} \qquad \frac{OpExpense_t}{OpExpense_{t-1}} = \frac{SR_t}{SR_{t-1}}$$

$$\text{(ii)} \qquad OpExpense_{t-1} = \alpha \cdot NOA_{t-1}$$

$$\text{(iii)} \qquad OpExpense_t = \alpha \cdot \frac{NOA_{t-1}}{SR_{t-1}} \cdot SR_t$$

Proof: Elementary algebra.

Statements (i) and (ii) represent an accounting under ideal circumstances; (i) relates to two adjacent periods and (ii) relates to how the period expense and the carrying value constitute two sides of the coin. Of course, "ideal circumstances" do not apply in general and thus one obtains the rule (*) by combining (i) and (ii). But (*) by itself does not of course imply either (i) or (ii), except by chance.

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[3] A slightly different version of this Proposition is: Any two of the following three statements imply the third:

(i) $OA_t / OA_{t-1} = SR_t / SR_{t-1}$
(ii) $OpExpense_t = \alpha \cdot OA_t$
(iii) the rule (*).

The rule (*) causes the accounting to satisfy an adjustment process. In other words, the accounting over time, works such that (i) and (ii) act as targets to which the accounting gravitates. That is, if the current carrying value OA_t seems too high relative to the current sales or expense, then one can expect a decline in the next period's profit margin. The growth in the expense has to be speeded up. Such reasoning is familiar from "regular" inventory accounting and, perhaps more pertinent, oil and gas accounting under the so-called full-costing approach.

The idea of (i) and (ii) being statements of an equilibrium comes into focus given the rule (*). If (*) applies and (i) happens to be true, then (ii) also holds. That is, if operating revenues and matched expenses grow at the same rate, then the expenses are in equilibrium relative to the underlying carrying value of the operating assets. Similarly, if the "ideal condition" (ii) happens to be true, then the operating expenses and sales must grow at the same rate.

5. Establish the accounting rule parameter α.

In the simplest possible accounting one conceptualizes α as constant corresponding to the inverse of the average useful life of a dollar of expenditure. This way of looking at a dollar of expenditure is of course familiar from depreciation. But here we need to qualify the depreciation metaphor because (i) expenses are matched with sales revenues as opposed to being purely periodic and (ii) we do not visualize that the accounting can change α for one period to the next (iii) there can be no write-offs of the balance in operating assets. Though in these three regards the accounting differs practically and conceptually from traditional depreciation accounting, α^{-1} does indeed generally measure the expected useful life of a dollar of sales sustaining expenditure. In the Proposition, statement (ii) underscores this point.

The concept of "the expected useful life of an average dollar of expenditure" may seem shaky. One can ask the question: What happens if there is a change in the business strategy so that the average useful life changes? One can deflect the critique inherent in the question by generalizing the accounting in a fairly straightforward manner. Specifically, the expenditures can be split in "pools," and where each pool allows for a range of expected useful lives.

For example, consider the following three types of pools:

(a) Expenditures expenses as incurred (or $\alpha = \infty$). Regular Selling & Administration expenditures, such as salaries and wages naturally fall into this category.

(b) Expenditures with a relatively long expected useful life, five years or more, say: capital expenditures (net of dispositions), purchased goodwill, and R&D expenditures would naturally fall into this pool. So do many kinds of advertising expenditures.

(c) Expenditures of an intermediate expected useful life. One can think of this pool as including all expenditures that have not been put into either (a) or (c). Expenditures related to acquisition of inventories would implicitly be placed in this pool. The same can be said for advertising expenditures that are judged to benefit only the near future.

As another example, which specifies only two pools, consider the following:

(a) All expenditures that have long-term benefits, such as regular capital expenditures, R&D, advertising, and purchased intangible assets, including goodwill. These expenditures can than be presumed to have an average useful life of, say, 15 years.

(b) All other sales sustaining expenditures. These expenditures can then be presumed to have an average useful life of, say, one year.

As a final comment, note that the measurement of operating earnings, or its components, need not be sensitive to choices of α. After all, in a steady state the accruals will be zero regardless of the number of pools and their associated α's. More generally, the basic idea with the accounting is simply that firms that grow by "investing in the future" will have positive accruals. With the passage of time these will have to be expensed in the spirit of all operating assets and effectively deferred expenses.

IV. SUMMARY

For the proposed accounting, though in its essence not particularly complicated, it may be useful to remind the reader about the accounting's central characteristics. In general, we view these as both practical and appealing. That said, we must stress that the word "appealing" is admissible only because the approach here solely focuses on the income statement and the search for a bottom line, namely, "earnings due to operating activities."

- The accounting identifies two distinct, mutually exclusive activities, namely, operating versus financial activities.
- The accounting provides a measure of "cash earnings," which, in turn, can be split into its operating and financial components.
- The accounting's concept of cash earnings develops from approximate market-value equivalent assets/liabilities and dividends net of capital contributions.
- The accounting splits the total operating expenses into its two components, the operating (approximate) cash expenditure and the total net accrual.
- The accounting results in a smoothed measure of operating earnings by disallowing discrete nonrecurring items such as write-downs and realized gains/losses due to sale of operating assets.
- The accounting requires no distinction between purchased goodwill and other capital expenditures (including R&D). All such expenditures can be pooled and expensed as a package.
- The accounting admits a break-down of the sale sustaining expenditures into several pools.
- The accounting addresses the issue of how expenses can be matched with sales via the rule (*).

Editorial Data

The following table contains information about turnaround time for manuscripts (including revisions) on which editorial decisions were made in the 12-month period ended August 31, 2005. Turnaround time is the number of days between the date that the manuscript was received *and the date of the editor's letter to the author(s)*. The number in parentheses reflects June–August 2005, under incoming Editor Dan Dhaliwal.

	Number of Manuscripts	Percent	Cumulative Number	Cumulative Percent
0 ≤ Days ≤ 30	84 (11)	18.06	84	18.06
31 ≤ Days ≤ 60	183 (43)	39.36	267	57.42
61 ≤ Days ≤ 90	140 (26)	30.11	407	87.53
91 ≤ Days ≤ 120	49 (13)	10.54	456	98.07
121 ≤ Days	09 (2)	1.93	465	100.00
Total	465 (95)	100.00		

The mean review time was 55.93; the median review time was 77 days.

New Submissions by calendar year (January 1 to December 31)

1992	241
1993	234
1994	231
1995	195
1996	230
1997	195
1998	196
1999	239
2000	260
2001	260*
2002	324
2003	327
2004	307
2005 (1/1–8/31)	267** (8 months)

* 2001—in addition to the 260 regular submissions, 68 MSs were submitted to the *TAR* Quality of Earnings Conference.
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EDITORIAL POLICY AND STYLE INFORMATION

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Hyphens: Use a hyphen to join unit modifiers or to clarify usage. For example: *a well-presented analysis; re-form*. See *Webster's* for correct usage.

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An Abstract of about 100 words should be presented on a separate page immediately preceding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings. Keywords and the Data Availability statements should follow the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the Introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

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Fabozzi, F., and I. Pollack, eds. 1987. *The Handbook of Fixed Income Securities*. 2nd edition. Homewood, IL: Dow Jones-Irwin.

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———. 1984b. The perceived effects of tax policy on corporate investment intentions. *The Journal of the American Taxation Association* 6 (Fall): 7–19.

Shaw, W. H. 1985. Empirical evidence on the market impact of the safe harbor leasing law. Ph.D. dissertation, The University of Texas at Austin.

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JANUARY 2006 PLACEMENT ADS

The deadline for information to be included in the "Placement Information" section is two months prior to the date of publication. The ***Review*** *is published in January, April, July, and October. Placement advertising and replies should be sent to the American Accounting Association, 5717 Bessie Drive, Sarasota, FL 34233-2399, ATTENTION: ADVERTISING DEPT. All replies to box numbers should be on separate sheets to facilitate forwarding.*

101 UNIVERSITY OF UTAH, School of Accounting and Information Systems, is recruiting for tenure-track faculty in Accounting; pending funding, University approval. Candidates in any area of specialization, seeking Assistant/Associate rank are invited to apply. Appointment begins Fall 2006. Applicants should possess a doctorate or be close to completion. We offer competitive salaries, research support, and teaching loads, and excellent fringe benefits. The School of AIS and the David Eccles School of Business are accredited by the AACSB. Send application by January 15, 2006, including dissertation proposal or working paper, *vita*, and three letters of reference to Robert Allen, Director, School of AIS, 1645 E Campus Center Dr., Room 108, University of Utah, Salt Lake City, Utah 84112. Late applications accepted until position is filled. The University of Utah is an Equal Opportunity/Affirmative Action Employer, encourages applications from women and minorities, and provides reasonable accommodation to the known disabilities of applicants/employees.

102 DARTMOUTH COLLEGE, Tuck School of Business, at Dartmouth College's graduate school of management, anticipates faculty searches in accounting. We are open to all levels of appointment: Assistant, Associate, or Full Professor, however, there is a preference for those who have at least three years' experience since completing their Ph.D. We are interested in candidates who will produce research of excellent quality that will have high impact on the field. There must be evidence that the candidate can conduct research and teach at a level of quality consistent with Tuck's high standards. Send resumes to: Senior Associate Dean Robert G. Hansen, 100 Tuck Hall, Hanover, NH 03755. Dartmouth College is an Equal Opportunity/Affirmative Action Employer and has a strong commitment to diversity. Women, persons of color, persons with disabilities and veterans are encouraged to apply.

103 WESTERN MICHIGAN UNIVERSITY, Haworth College of Business Department of Accountancy invites applications for a tenure-track position in financial/managerial accounting at the Assistant or Associate Professor level beginning Fall 2006. Candidates must have a Ph.D. with an accounting major (or ABD with completion by end 2006). Salary is competitive. Teaching responsibilities would be at both the undergraduate and graduate levels. A reduced teaching load is available for new faculty. The Department's primary emphasis is on instruction, followed by a strong emphasis on intellectual contributions. We encourage faculty to engage in applied research, instructional development, and basic research, in that order. Service that advances the mission of the Department is also important. Please direct applications to Chair David Rozelle (david.rozelle@wmich.edu) Haworth College of Business, Department of Accountancy, Western Michigan University, Kalamazoo, Michigan 49008-5402. Western Michigan University is an Affirmative Action/Equal Opportunity Employer.

104 UNIVERSITY OF OKLAHOMA, School of Accounting anticipates a faculty opening for the academic year 2006–2007 at the rank of Assistant or Associate Professor. Candidates must possess an appropriate doctorate or be near completion. Candidates must demonstrate potential for research by their doctoral dissertation, publications, and working papers. Candidates must be able and willing to teach at both the undergraduate and graduate level. **Salary and Fringe Benefits**: Competitive commensurate with rank and experience. **Anticipated Appointment Date**: August 15, 2006 All qualified individuals are encouraged to submit application materials to: Professor Frances L. Ayres, School of Accounting, College of Business Administration, The University of Oklahoma, Norman, Oklahoma 73019-4004; Email: fayres@ou.edu; Phone: (405) 325-5768; Fax: (405) 325-7348. Applications will be accepted until the positions are filled. The University of Oklahoma is an equal Opportunity/Affirmative Action Employer. Women and Minorities are encouraged to Apply.

105 TEXAS A&M UNIVERSITY, Department of Accounting in the Mays Business School invites applications for an anticipated position beginning in August 2006. The position is open to rank but we have a strong preference for candidates at either the advanced Assistant or Associate Professor level. Candidates with teaching interests in auditing are preferred. Applicants should have a doctoral degree in accounting and should have a strong commitment to scholarly research and teaching excellence. Please send a cover letter and resume that includes: (1) educational achievement; (2) research/publication record; (3) indicators of teaching activity/effectiveness or potential; (4) work and other experience; and (5) a list of three references with addresses and phone numbers. All applications should be directed to: James Benjamin, Head, Department of Accounting, 4353 TAMU, College Station, TX 77843-4353 (or by email: j-benjamin@tamu.edu). Review of applications will begin on September 1, 2005 and will continue until the position is filled. Texas A&M University is an Equal Opportunity,/Affirmative Action Employer.

106 Major investment bank seeking financial executives to develop and introduce a new corporate valuation model targeted to their institutional clients. We are seeking an individual with substantial technical expertise in corporate valuation theory (Ph.D. preferred, CPA, CFA, or M.S. is also desirable, in a financial, accounting or quantitative discipline), with experience in product management and a deep understanding of corporate valuation theory and practice. Experience in either corporate valuation analysis, product management of a computer-based model and/or client services in a highly technical financial services arena is strongly desired. Position Available Date: August 23, 2005 Applicants should contact Brendan O'Brien at The White Rock Group. Email: MB90901@hotmail.com.

107 SUNGKYUNKWAN UNIVERSITY, Graduate School of Business, seeks applications for a tenure-track position (Assistant or Associate Professor) in Financial Accounting to begin in the Fall 2006. SKK GSB is a business school newly founded by Sungkyunkwan University with the assistance of the Samsung Group and in collaboration with the MIT Sloan School of Management. Our mission is to become the best M.B.A. program in Asia. Salary and teaching loads are highly competitive. Generous fringe benefits include free housing, education allowance for international schools, and medical insurance. All candidates must have a doctoral degree in Accounting prior to the appointment. Candidates should show their strong potential for and/or record of excellence in research and M.B.A. teaching. Excellent English communication skills are required. Applicants should submit a cover letter, an updated *curriculum vitae*, an example of written work for presentation, three letters of reference, evidence of teaching effectiveness to Dr. Jae Ha Lee, Associate Dean, SKK GSB, Sungkyunkwan University, 53 Myungryun-dong 3-ga, Jongro-gu, Seoul, Korea 110-745. Phone: +82-2-740-1505, Email: gsbfaculty@skku.edu. Review of applications will begin immediately and continue until the position is filled. Additional information about SKK GSB can be found at: http://gsb.skku.edu.

108 AUSTIN PEAY STATE UNIVERSITY, School of Business invites applications for the tenure-track positions of Assistant Professor of Accounting starting in the 2006–2007 year. Two positions will be filled. A Ph.D. or D.B.A. in Accounting or a closely related field is preferred. ABDs within six months of degree completion also considered. Applicants should also have evidence of or potential for research and publications. Primary needs are in Auditing, Accounting Information Systems, and Managerial Accounting. Applicants should be prepared to teach on both the undergraduate and masters levels. Experience with web-based teaching is desirable. Review starts September 2005 and will continue until the positions are filled. Application Procedure: Please submit letter of application; *curriculum vitae* (including your email address); and names, addresses, and telephone numbers of three references to: Office of Human Resources Austin Peay State University Attn: HR Faculty Applications, MSM Program PO Box 4698, Clarksville, TN 37044; Email: FacultyApplications@apsu.edu; Fax: (931) 221-7105. Minorities, women, and members of other protected groups are encouraged to apply. Austin Peay State University is an Affirmative Action/Equal Opportunity Employer.

109 INDIANA UNIVERSITY, Department of Accounting, invites applications for a tenure-track faculty position effective Fall 2006. Preference will be given to Assistant professors with teaching and research interests in auditing, but individuals at all ranks are welcome to apply. Candidates must possess a doctoral degree and have a strong commitment to excellence in scholarly research and teaching. Compensation levels are competitive and negotiable. Interested candidates should send a current *vita*, a research paper suitable for presentation at a workshop, and evidence of teaching ability to the contact person indicated below. Three letters of reference will also be required before an offer of employment can be considered. Applications received before January 1, 2006 will be given full consideration, but the position will remain open until a suitable applicant is found. Contact address: Joe Fisher, Chair, Department of Accounting, FedEx Faculty Fellow, Kelley School of Business, Indiana University, 1309 E. 10th Street, Bloomington, IN 47405-1701. Phone: (812) 855-3705. Email: jofisher@indiana.edu. Minorities, women, and persons with disabilities are encouraged to apply.

110 UNIVERSITY OF NOTRE DAME, Department of Accountancy, invites applications for position openings at the Assistant and Associate Professor levels. All research areas will be considered. Applicants for Assistant Professor positions should expect to complete doctoral degree requirements before the start of the contract period. All candidates should have a strong commitment to scholarly research and excellence in teaching. Salary is very competitive and research support is excellent. Please send *curriculum vitae* and an example of scholarly work to Professor Tom Schaefer, Chair, Department of Accountancy, Room 102D, Mendoza College of Business Administration, University of Notre Dame, Notre Dame, IN 46556-5646. The University of Notre Dame is an Equal Opportunity/Affirmative Action Employer.

111 KENT STATE UNIVERSITY, Department of Accounting, invites applications for an Assistant Professor with teaching emphasis primarily in Managerial Accounting beginning in Fall 2006. Individuals with interest in Financial Accounting will also be considered. Hiring of an applicant with a quality, established research record is preferred. The successful candidate may also serve as a KPMG Faculty Fellow. A Ph.D. with a major in Accounting or an appropriate related field is required. Professional certification such as CMA or CPA is highly desirable. The preferred candidate should be able to demonstrate evidence of discovery/integration research and of effective teaching. Research will be expected in high-quality, academic refereed journals. Service on and/or supervising doctoral students' dissertations will be part of the faculty member's responsibilities. Review of applications will begin immediately and continue until the position is filled. If you are interested in this position, please contact and/or mail a copy of your resume to: Dr. Pervaiz Alam, Chair, Faculty Search Committee Department of Accounting, 576 BSA, Kent State University, Kent, OH 44242; Phone: (330) 672-1121; Fax: (330) 672-2548; Email: palam@bsa3.kent.edu.

112 GEORGIA COLLEGE AND STATE UNIVERSITY, Department of Accounting, invites applications for Chair of the Department. The J. Whitney Bunting School of Business is AACSB-accredited and has 38 full-time faculty members. The Chair is responsible for promoting quality teaching, fostering research, and maintaining excellence in all programs. The Chair will also teach a reduced load of undergraduate and graduate courses. Applicants should possess a Ph.D. or D.B.A. in Accounting and have demonstrated excellence in teaching and a record of scholarly accomplishments. The appointment will begin January 1, 2006, July 1, 2006, or at a time agreeable to all parties. Submit a letter of application, *vitae*, and the names of three references to Dr. Tom Moore, Search Committee Chair, Campus Box 015, 231 West Hancock St., Georgia College and State University, Milledgeville, Georgia 31061; Email: tom.moore@gcsu.edu. An Equal Opportunity/Affirmative Action Employer.

113 Position Desired—Areas of Interest: Principally Financial Accounting/Reporting, also Management Accounting, Taxation, and AIS. Seeking sponsorship to emigrate to the U.S. My main qualifications are a bachelor's degree, a U.K. accountancy qualification (FCCA—U.K. equivalent of CPA) and a master's degree from Oxford University. Several years as an accountancy academic at a leading U.K. university with primarily undergraduate teaching experience, several publications, and proceedings. Big 4 experience. Start date Fall 2006. Email: brianhenderson111@yahoo.co.uk.

114 UTAH STATE UNIVERSITY, School of Accountancy (SOA) invites applications for the George S. Eccles Chair in Capital Markets Research at the rank of Associate or Full Professor beginning Fall Semester 2006. The candidate must have an earned doctorate degree and be an active, nationally recognized scholar with significant contributions to capital markets research. Evidence of outstanding classroom teaching is also required. USU, a Research Extensive Institution located in Logan, Utah, has a student body of 22,000. The SOA is separately accredited by the AACSB. Screening of applicants will commence December 1, 2005, and continue until a suitable candidate is found. Please send letter of application, current *vita*, and contact information for three professional references to: Dr. Rosemary Fullerton, Chairperson, Endowed Chair Search Committee, School of Accountancy, Utah State University, Logan, UT 84322-3540; Email: Rosemary.Fullerton@usu.edu. For additional information, please visit http://www.usu.edu/hr/jobsbusiness.htm. AA/EOE.

115 THE UNIVERSITY OF NORTH CAROLINA AT CHAPEL HILL, Kenan-Flagler Business School, is seeking to hire one or more faculty members for a full-time, tenured, tenure-track, or fixed-term position in the Accounting Area. Applicants should be able to teach in at least one of the following areas: financial accounting, managerial accounting, taxation, or auditing. Applicants should have or be near completion of a Ph.D. with an emphasis in accounting and a demonstrated commitment to research. Applicants should have teaching experience as well as significant research accomplishments. All applicants should be able to contribute to the University's teaching mission at the undergraduate, Master of Accounting, M.B.A., Ph.D., and Executive Education levels. Interested individuals should write, enclosing *curriculum vitae*, a sample of research papers and four letters of recommendation to: Professor Edward Maydew, Chair of Accounting Area, Kenan-Flagler Business School, University of North Carolina at Chapel Hill, McColl Bldg, CB 3490, Chapel Hill, NC 27599-3490. Website: http://www.kenan-flagler.unc.edu/. The University of North Carolina at Chapel Hill is an Equal Opportunity Employer.

116 OGLETHORPE UNIVERSITY, a selective private liberal arts institution located in Atlanta, Georgia, invites applications for a tenure-track faculty position in Accounting beginning Fall 2006. Ph.D./D.B.A. in Accounting or CPA with Master's degree required. All areas of accounting considered, but preference given to candidates with teaching interests in Auditing and/or Systems. Commitment to excellence in teaching, service, active involvement in campus life, and scholarly activity are expected. Please submit a letter of application, *curriculum vitae*, transcripts, and three letters of recommendation to Dr. Dean Tucker, Oglethorpe University, 4484 Peachtree Rd. NE, Atlanta, GA 30319-2797, preferably before November 15, 2005; Email: DTucker@oglethorpe.edu. AA/EOE.

117 BINGHAMTON UNIVERSITY AT SUNY, AACSB's accredited School of Management invites applications for two Assistant Professor positions in Accounting for Fall 2006. The positions are subject to budgetary approval. Preference will be given to applicants with teaching and research interests in any of financial accounting, managerial accounting, or auditing, but all interested candidates are invited to apply. Candidates must have (or expect) a Ph.D. degree and demonstrate the ability to conduct a nationally recognized research program. Quality research is supported with a two course/semester teaching assignment. Located three hours from New York City and one hour from Syracuse and Ithaca, Binghamton is a low-cost-of-living regional medical/retail hub for 300,000 people. Interested candidates should send a *vita* and other supporting documents to: Professor Steven Schwartz, School of Management, Binghamton University PO Box 6000, Binghamton, NY 13902-6000; Fax: (607) 777-4422; Email: sschwart@binghamton.edu. Binghamton University is an Equal Opportunity/Affirmative Action Employer.

118 YORK UNIVERSITY, Atkinson Faculty of Liberal and Professional Studies, School of Administrative Studies invites qualified applications for tenure-track positions in the financial and managerial accounting and tax areas starting July, 2006. Detailed position descriptions are available at: http://www.yorku.ca/acadjobs. Reply to: John Parkinson, Room 223, Atkinson Building, York University, 4700 Keele Street, North York, ON M3J 1P3, Canada. York University is an Affirmative Action Employer. The Affirmative Action Program can be found on York's website at www.yorku.ca/acadjobs or a copy can be obtained by calling the affirmative action office at (416) 736-5713. All qualified candidates are encouraged to apply; however, Canadian citizens and Permanent Residents will be given priority.

119 UNIVERSITY OF TEXAS–PAN AMERICAN, Department of Accounting and Business Law in the College of Business Administration invites applications for three full-time tenure-track faculty positions in Accounting at the rank of Assistant/Associate Professor beginning Fall 2006. Required: a doctorate in accounting from an AACSB-accredited institution (ABDs considered at the Assistant Professor level with Ph.D. in hand by August 31, 2006). Candidates should have the potential to develop an outstanding record in teaching, scholarship, and service. Business experience in the field and professional certification are desired. Salaries are competitive; position open until filled. Send a letter of application and a complete resume to: Dr. Kent Fields, Chair, Search Committee Department of Accounting & Business Law, College of Business Administration, University of Texas–Pan American, 1201 West University Drive Edinburg, TX 78541-2999. UTPA is an Equal Opportunity/Affirmative Action Employer. Women and minorities are encouraged to apply. This position is security sensitive as defined by the Texas Education Code §51.215 (c) and Texas Government Code §411.094(a) (2). For more information please see http://www.utpa.edu.

120 UNIVERSITY OF EVANSVILLE, School of Business Administration invites applications for a tenure-track position in Accounting at the Assistant/Associate Professor level beginning Fall 2006. This is a new faculty position funded by a $15 million grant from the Lilly Endowment, Inc. Teaching interests in financial accounting with secondary interests in finance/economics are preferred. Professional experience and certification are desirable. Knowledge of global business applications and experience is a plus. An earned doctorate in accounting is preferred, along with demonstrated research potential. Applications will be accepted until November 15, 2005 or the position is filled. Send letter of application, *vita*, and three letters of reference to: Dr. James Schaefer, School of Business Administration, University of Evansville, 1800 Lincoln Ave., Evansville, IN 47722, or electronically to: js2@evansville.edu. Persons from traditionally underrepresented groups are strongly encouraged to apply.

121 UNIVERSITY OF ILLINOIS AT SPRINGFIELD invites applications for tenure-track positions in Accountancy at Assistant or Associate rank to begin January or August 2006. Appointment at the Associate Professor rank will be consistent with achievements of the candidate, with expectations of an Associate professor in teaching and research. Candidates should have a Ph.D. or (in exceptional cases) ABD, with a requirement of completion within one year of joining the faculty; CPA, CIA, or CMA is required for all advanced ranks. Review of applications begin September 2005 and will continue until positions are filled. Submit a letter of application, *vita*, and names of three references to: Bonnie Moe, Accountancy, College of Business and Management, University of Illinois at Springfield, One University Plaza, Springfield IL 62703-5407 or email: moe.bonnie@uis.edu. The University of Illinois at Springfield is an Affirmative Action/Equal Opportunity Employer. Minorities, women, veterans, and persons with disabilities are encouraged to apply.

122 THE UNIVERSITY OF TEXAS AT DALLAS invites applications for a faculty position in Accounting, effective September 2006. Appointment at Assistant, Associate, or Full Professor level will be considered. Appointees must have (or expect) a Ph.D. or equivalent and demonstrate the ability to conduct a nationally recognized research program. Applicants should send a complete *curriculum vitae* and names and addresses of three references to: Academic Search 4117, The University of Texas at Dallas, PO Box 830688, AD 23, Richardson, TX 75083-0688. Indication of ethnicity and sex are requested as part of the application, but not required. UTD is an AA/EO University and strongly encourages applications from candidates who would enhance the diversity of the university's faculty and administration. For more information, visit http://www.som.utdallas.edu/aim or contact Ashiq Ali, Ashbel Smith Professor, (972) 883-6360, ashiq.ali@utdallas.edu.

123 METROPOLITAN STATE COLLEGE OF DENVER is seeking Tenure-track Faculty for 2 positions: F002 and F014. Please go to http://www.mscd.edu/news/jobs.htm for more information and application instructions for this position. Teach 12 hours (four courses) of day, evening, and/or weekend classes per semester, including at least one advanced course. Other duties include: professional development; student advising; departmental, collegiate and community service; participation in course and program development; and other tasks as assigned by the chair. The Department requires, in addition to the professional development in the area of specialization, a candidate able to relate philosophy to a liberal education and to contribute to the intellectual life of a small but vigorous department. Qualifications: Required: Doctorate or ABD in the teaching field with proof that the dissertation topic has been approved. Preferred: Ph.D. or a Doctorate in Business Administration with an Accounting major awarded from an AACSB-accredited institution. A relevant professional certification. A record of intellectual contributions, service, and excellence in teaching. Appointment: Begins August 2006. Metropolitan State College of Denver is an Equal Opportunity Employer and encourages women and minorities to apply.

124 UNIVERSITY OF FLORIDA, Fisher School of Accounting seeks junior faculty for the 2006–2007 academic year. Candidates should have a strong potential in research and teaching. Ph.D. required for tenure-track. Salaries are competitive and vary with credentials of appointee. Women and minorities are encouraged to apply. For consideration, applicants should send a *vitae* and representative research by February 14, 2006 to: Professor Joel S. Demski, Associate Dean and Director, Fisher Eminent Scholar, Fisher School of Accounting University of Florida, 210 Gerson Hall, PO Box 117166, Gainesville, FL 32611-7166. The University of Florida is an Equal Opportunity/Affirmative Action Employer. The selection process will be conducted under the provisions of Florida's Government in the Sunshine and Public Records law.

125 THE GEORGE WASHINGTON UNIVERSITY, Department of Accountancy invites applications for one non-tenure-track position at the rank of Assistant Professor. The appointment will be for an initial term of up to three years. The anticipated starting date is Fall 2006. Applicants for the position are expected to have completed all requirements for a relevant doctoral degree by August 1, 2006 in order to be considered for appointment at the rank of Assistant Professor. ABDs will also be considered for appointment, and if hired, will be appointed at the rank of Instructor, on the condition that the doctoral degree be completed by the end of the first academic year of appointment. Candidates should have a strong commitment to teaching excellence, scholarly research, and institutional service. Please send a letter of application, a current resume, and current working papers to: Professor, Chair, Department of Accountancy, School of Business, The George Washington University, Washington, D.C. 20052. Electronic applications may be sent to accyjob@gwu.edu. Review of applications will begin on December 28, 2005 and continue until the position is filled. The George Washington University is an Equal Opportunity/Affirmative Action Employer.

126 UNIVERSITY OF SCRANTON, Kania School of Management, AACSB-accredited, is seeking qualified applicants for a tenure-track position beginning August 2006. The University offers courses at the undergraduate and M.B.A. levels. Candidates should have a teaching interest in Taxation, Law, Auditing, Accounting Systems, or Financial Accounting. The qualifications include an appropriate earned doctorate or ABD progressing toward completion. The University of Scranton in northeastern Pennsylvania is one of 28 Jesuit colleges and universities in the United States committed to providing liberal arts education and strong professional and pre-professional programs in the context of Ignation educational principles. The University is proud of its mission in the Catholic and Jesuit tradition and spirit, and the successful candidate must be able to support this mission through his or her work in Accounting. Applications will be received until the position is filled. Please send *curriculum vitae* to: Dr. Robyn Lawrence, Chair, Department of Accounting, University of Scranton, Scranton, PA 18510-4602; Email: accounting@scranton.edu. The University is an Affirmative Action/Equal Opportunity Employer/Educator; women and minorities are encouraged to apply.

127 UNIVERSITY OF SCRANTON Kania School of Management, AACSB-accredited, is seeking qualified applicants for a tenure-track position beginning August 2006. The university offers courses at the undergraduate and M.B.A. levels. The primary teaching responsibility will be Auditing. The qualifications include an appropriate earned doctorate or ABD progressing toward completion. The University of Scranton in northeastern Pennsylvania is one of 28 Jesuit colleges and universities in the United States committed to providing liberal arts education and strong professional and pre-professional programs in the context of Ignation educational principles. The University is proud of its mission in the Catholic and Jesuit tradition and spirit, and the successful candidate must be able to support this mission through his or her work in Accounting. Applications will be received until the position is filled. Please send *curriculum vitae* to: Dr. Robyn Lawrence, Chair, Department of Accounting, University of Scranton, Scranton, PA 18510-4602; Email: accounting@scranton.edu. The University is an Affirmative Action/Equal Opportunity Employer/Educator; women and minorities are encouraged to apply.

128 SANTA CLARA UNIVERSITY, Department of Accounting is authorized to hire two tenure-track Assistant, Associate, or Full Professors for Fall 2007. Candidate's primary teaching interest should be financial accounting, managerial accounting, or information systems. Candidates for Assistant Professor should have completed/nearly completed their Ph.D. and be committed to a scholarly research program. Candidates at higher ranks should have a completed doctorate and a continuing record of nationally recognized scholarly research and quality teaching. Reviews of applications will begin October 1, 2006, but informal inquiries are welcome before that date. If you are interested in learning more about this position, contact Dr. Michael Calegari, Department of Accounting, Leavey School of Business, Santa Clara University, Santa Clara, CA 95053-0380 (or email: mcalegari@scu.edu). SCU is a private Jesuit university located in Silicon Valley with competitive salaries, housing subsidies, and summer research support. SCU is an EEO/AA employer.

129 SUFFOLK UNIVERSITY, Sawyer School of Management invites applications for two tenure-track accounting positions for Fall 2006. We need faculty in the areas of tax and financial reporting, although all areas of research expertise will be considered. Both entry-level and senior-level appointments will be considered. The Accounting Department is accredited by AACSB International. Among the programs offered and supported by the Accounting Department are the B.S.B.A. with an accounting major, the M.S.A., the M.S.T., joint B.S.B.A./M.S.A. and B.S.B.A./M.S.T. programs, and the M.B.A. with an accounting concentration. Successful candidates will be strongly committed to being excellent teachers and productive scholars. A doctoral degree (or ABD) is required and professional qualification is preferred. Salary is competitive. Submit *vita*, names of three references, and a letter outlining teaching and research interests to: Professor Ruth Ann McEwen, Chairperson, Accounting Department, Frank Sawyer School of Management, Suffolk University, 8 Ashburton Place, Beacon Hill, Boston, MA 02108-2770; Email: ruthann@ruthannmcewen.com. Suffolk University is an Equal Opportunity Employer; applications from minority candidates are encouraged.

130 UNIVERSITY OF MASSACHUSETTS BOSTON invites applications for tenure-track positions beginning Fall 2006. Candidates with primary interests in Accounting Information Systems are desired; however, candidates in all areas of interest are encouraged to apply. A Ph.D. in Accounting is required, but ABDs nearing completion will be considered. Send *curriculum vitae* to: Arindam Bandopadhyaya, Chair, Department of Accounting and Finance, College of Management, University of Massachusetts Boston, 100 Morrissey Boulevard, Boston, MA 02125-3393. UMass Boston is an Equal Opportunity/Affirmative Action Employer.

131 CENTRAL CONNECTICUT STATE UNIVERSITY invites applications for tenure-track positions in accounting at the Assistant, Associate, or Full Professor level beginning in or after Spring 2006. Applicants must have an earned doctorate in accounting from an AACSB-accredited school or equivalent. Professional certification or experience is desirable. Experience and credentials above this level may be eligible for consideration at the level of Associate or Full Professor. Teaching interest in financial, auditing, or AIS preferred. Applicants must have a commitment to excellence in teaching and scholarship. Review will continue until the position is filled. Review of applicants will begin on October 15, 2005, and continue until positions are filled. Send cover letter and resume electronically to: Ms. Donna Stewart at: stewartd@ccsu.edu, Department of Accounting, School of Business, Central Connecticut State University, 1615 Stanley St., New Britain, CT 06050. CCSU is an Equal Opportunity Employer and specifically invites applications from women and minorities.

132 RENSSELAER POLYTECHNIC UNIVERSITY invites applications for a tenure-track position at the rank of Assistant or Associate Professor. However, all areas will be considered. Ph.D. in Accounting or related field and evidence of research potential and teaching effectiveness are required. Candidates for the rank of Associate Professor should have a proven research record. The appointment involves teaching at both the undergraduate and graduate levels. Position available: August 1, 2006. Salary: Competitive. Contact: Applicants should send a *c.v.*, names of three references, and a sample of research work to: Ms. Enza Bove, Accounting Position, The Lally School of Management and Technology, Rensselaer Polytechnic Institute, 110 8th Street, Troy, New York 12180-3590. FMA Yes Iftekhar Hasan, Bill Francis; SFA Yes Iftekhar Hasan, Bill Francis. Rensselaer Polytechnic Institute is an Employment Equity/Affirmative Action Employer.

133 PACE UNIVERSITY, Accounting Department of the Lubin School of Business, seeks an exceptional scholar to head the newly created Center for Financial Reporting in Global Markets. The Center, through its Director, will respond to the increasing need for clarity and knowledge in this area by acting as a catalyst for research and case study development on issues in financial reporting. Candidates must meet all the requirements for appointment as an Associate or Full Professor and have a distinguished record of research in the field of accounting. The Lubin School of Business is fully accredited by the AACSB International. Salary and benefit package is competitive. Please submit your *curriculum vitae* to: Rudolph Jacob, Chair, Accounting Dept., Pace University One Pace Plaza, New York, NY 10038. Visit us at http://www.pace.edu. Pace University is an Affirmative Action/Equal Opportunity Employer that values and encourages diversity in it its students, faculty, and staff.

134 UNIVERSITY OF CALIFORNIA, DAVIS, Graduate School of Management invites applications for one tenure-track position in Accounting. The position will be filled at the Assistant Professor level with a preference for those who have at least two years' experience since completing their Ph.D. Applications should demonstrate excellence or potential excellence in teaching managerial accounting, research interests open. Recently ranked as one of the top public M.B.A. schools in the nation, the GSM offers an exciting intellectual environment of top research scholars and outstanding professional students. Candidates should have a Ph.D. degree in accounting or related field. Your letter of application should describe your research and teaching interests and include a resume, evidence of research, (papers, dissertation) and teaching, and the names and addresses of three references to: Faculty Search Committee, University of California, Davis, Graduate School of Management, One Shields Ave., Davis, CA 95616; Email: FacultyRecruit@gsm.ucdavis.edu. This position has no closing date and, as such, will remain open until filled. However, the Committee expects that an initial consideration of candidates' applications will be completed no later than December 31, 2005. The University of California is an Affirmative Action/Equal Opportunity Employer with a strong institutional commitment to the achievement of diversity among its faculty and staff.

135 UNIVERSITY OF MISSOURI, ST. LOUIS College of Business Administration invites applications for two positions of Assistant Professor of Accounting, beginning August 2006. Applicants should possess a doctorate or be near completion. The successful candidate should have a strong commitment to excellence in teaching and research. The College offers undergraduate and master's degree programs including a B.S. in Accounting, a Master of Business Administration and a Master of Accounting. All programs are AACSB-accredited. Interested candidates should contact: Mary Beth Mohrman, Associate Professor and Coordinator of Accounting, College of Business Administration, University of Missouri, St. Louis, 8001 Natural Bridge Road, St. Louis, MO 63121; Email: mohrman@umsl.edu. The University is an Affirmative Action/Equal Opportunity Employer committed to excellence through diversity.

136 UNIVERSITY OF TENNESSEE, KNOXVILLE, Department of Accounting and Information Management, invites applicants for a visiting position for the academic year beginning Fall 2006. All academic ranks will be considered. Applicants must have teaching interests in information systems, e-commerce, databases, and spreadsheet techniques. In addition, applicants must possess research skills necessary to contribute to the research mission of the department. Review of applications will continue until the position is filled. Please submit applications to: Kenneth E. Anderson, Search Committee Chair Department of Accounting and Information Management, CBA, University of Tennessee, Knoxville, TN 37996-0560; Email: kea@utk.edu. UT Knoxville is an EEO/AA/Title VI/Title IX/Section 504/ADA/ADEA institution in the provision of its education and employment programs and services. The University welcomes and honors people of all races, genders, creeds, cultures, and sexual orientations, and values intellectual curiosity, pursuit of knowledge, and academic freedom and integrity.

137 UNIVERSITY OF WISCONSIN–WHITEWATER invites applications for a full-time, academic staff position in Accounting for the academic year beginning August 27, 2006. Requirements are (1) a graduate degree in accounting or a related area, (2) professional experience in financial accounting and cost/managerial accounting, and (3) professional certification in accounting (e.g., CPA or CMA) or related field (e.g. CFM or CFA). A completed credential packet must include the following: (1) letter of application, (2) statement of teaching philosophy, (3) resume, (4) summary of teaching evaluations, and (5) three letters of recommendation or names and phone numbers of references, and (6) undergraduate and graduate transcripts. Applications received by January 31, 2006 are ensured review; position open until filled. If interested, please contact: Alka Arora, Chair Search and Screen Committee, Accounting Department, University of Wisconsin–Whitewater, 800 West Main Street, Whitewater, WI 53190-1790. The University of Wisconsin–Whitewater is an Affirmative Action/Equal Opportunity Employer.

138 UNIVERSITY OF WEST FLORIDA is seeking applications for a tenure-track faculty position at its Ft. Walton campus at the Assistant/Associate Professor level for Fall 2006. Candidates should have a primary teaching interest in financial accounting and auditing, and demonstrate an interest in conducting publishable research. Applications are encouraged from experienced Assistant/Associate Professors as well as new Ph.D.s or those nearing completion of their Ph.D. Preference will be given to persons with professional certification and accounting experience. Nominations should be sent to: Search Committee, College of Business, 11000 University Parkway, Pensacola, FL 32514. Applications are made online at https://jobs.uwf.edu, and include a letter of application; (2) a current *curriculum vita*; (3) two letters of recommendation; (4) evidence of excellence in teaching; and (5) evidence of research activity. For additional information, contact Mrs. Mary Holladay at (850) 474-3352, or mholladay@uwf.edu. Screening will begin December 1, 2005, and continue until the position is filled.

139 UNIVERSITY OF WEST FLORIDA invites applications for the Mary Ball Washington Endowed Chair in Accounting, beginning in August 2006. The appointment will be for a one to three year period with the possibility of renewal. The successful candidate should have a Ph.D./D.B.A. in Accounting from an AACSB-accredited university, an outstanding research record, distinguished teaching at both the undergraduate and graduate levels, and an exceptional service record. All areas in accounting will be considered. Preference will be given to persons with professional certification and accounting experience. Nominations should be sent to: Search Committee, College of Business, 11000 University Parkway, Pensacola, FL 32514. Applications are made online at https://jobs.uwf.edu, and include a letter of application; (2) a current *curriculum vita*; (3) two letters of recommendation; (4) evidence of excellence in teaching; and (5) evidence of research activity. For additional information, contact Mrs. Mary Holladay at (850) 474-3352 or mholladay@uwf.edu.

140 FLORIDA STATE UNIVERSITY, Department of Accounting, invites applications for tenure-track positions at the Assistant Professor or Associate Professor level beginning Fall 2006. Candidates must have an interest in teaching in the auditing and/or forensic accounting areas. Candidates must have a Ph.D. in accounting, and applicants at the Associate Professor level should have a well-established record of publications in high-quality journals and of effective teaching. Expectations include commitment to excellence in research, teaching, and service, including working with Ph.D. students. Applicants should send their *vita* to: Bud Fennema, College of Business, Florida State University, Tallahassee, FL 32306-1110; or email: bfennema@cob.fsu.edu.

141 THE UNIVERSITY OF TEXAS AT SAN ANTONIO, Department of Accounting invites applications for one senior lecturer and three tenure-track positions at the Assistant, Associate or Professor of Accounting levels beginning Fall 2006, pending budget approval. Responsibilities may include teaching undergraduate, masters, and/or Ph.D. courses offered at the Loop 1604 Campus or at the Downtown Campus. Some classes may be offered in the evenings. Applications from faculty interested in all areas of teaching and research in accounting are welcome to apply. Preferential status for the tenure-track positions will be given to tenurable faculty who have worked closely with Ph.D. students. For details concerning specific qualifications and application requirements, please consult the full text of the ad at http://business.utsa.edu/jobs/dept_acc2.htm. UTSA is an Affirmative Action/Equal Opportunity Employer. Women and minorities are encouraged to apply.

142 THE GEORGE WASHINGTON UNIVERSITY, Department of Accountancy invites applications for one non-tenure-track position at the rank of Assistant professor. The initial appointment is for up to three years starting Fall 2006. For appointments at the Assistant Professor level, applicants should have completed all doctoral degree requirements by August 1, 2006. ABDs will be considered at the rank of Instructor and, if appointed, must complete the doctoral degree by the end of the first academic year of appointment. A strong commitment to teaching excellence, scholarly research, and institutional service is expected. Please send the application, a current resume and working papers to: Professor, Chair, Department of Accountancy, School of Business, The George Washington University, Washington, D.C. 20052. Send electronic applications to: accyjob@gwu.edu. Application review begins on December 28, 2005. The George Washington University is an Equal Opportunity/Affirmative Action.

143 NATIONAL UNIVERSITY OF SINGAPORE, Department of Finance and Accounting, NUS Business School (http://www.bschool.nus.edu), invites applications for accounting tenure-track positions at all ranks (Assistant/Associate/Full) beginning August 2006. All accounting-related disciplines will be considered. Candidates should possess a Ph.D. or be close to completion. Our accounting faculty is active in research, having published in the recent past in such leading journals as *The Accounting Review*, *Journal of Accounting Research*, and *Contemporary Accounting Research.* The school has an extensive collection of databases, including Compustat, CRSP, and Datastream. Funding for research and international conference travels is generous. A reduced teaching load is provided to new staff. We offer salaries competitive with leading U.S. schools. Subsidized housing and educational allowances are also provided. To view complete job posting: http://www.bschool.nus.edu/Departments/FinanceNAccounting/weAreHiring.htm. Please send your *curriculum vita* and three letters of recommendation to: Associate Professor Michael SHIH, NUS Business School, 1 Business Link, Singapore 117592; Email: fnbbox1@nus.edu.sg.

144 YONSEI UNIVERSITY, School of Business (YSB), invites application for non-tenured accounting faculty positions. Contract periods are flexible: Six weeks in the summer, one semester, or one year and renewable—best suited for a faculty member who plans to take a sabbatical leave from the current position and wishes to experience the academic researches and business practices at Korea. All areas and ranks will be considered. Salary will be highly competitive, commensurate with qualifications and experience. YSB will provide round-trip airfare and on-campus residence. Please send letter of application and current *curriculum vitae* to: Professor Won-Wook Choi, School of Business, Yonsei University, Shinchon-dong, Seoul 120-749, Korea; Email: wonchoi@yonsei.ac.kr. For more information about YSB, please visit http://ysb.yonsei.ac.kr.

145 THE UNIVERSITY OF AKRON, School of Accountancy invites applications for a tenure-track appointment at the Assistant or Associate Professor level beginning January or August 2006. Candidates' primary teaching and scholarship interests should be taxation. Ability to teach in other areas is a plus. Expectations include teaching M.Tax courses, conducting publishable research, and playing a leadership role in the M.Tax program. Preference will be given to candidates with professional certification, taxation experience, and ability to work closely with the professional community. A Ph.D. is the minimum academic credential. We will also consider individuals with credentials that combine J.D., master's degree in a business field, and professional certification. Review of applications will be ongoing until the position is filled. Applicants should send a cover letter, names of three references, and *vita* to: Thomas Calderon, College of Business Administration, The University of Akron, Akron, OH 44325-4802; Phone: (330) 972-6228; Email: tcalderon@uakron.edu.

146 UNIVERSITY OF ILLINOIS AT CHICAGO invites applications for tenure-track positions at Assistant or Associate Professor levels from candidates interested in all areas of accounting, especially tax and auditing, beginning August 2006. Applicants should possess a Ph.D. and exhibit capacity to produce quality research and commitment to teaching excellence. Visit http://accounting.cba.uic.edu/ for further information. UIC is one of 70 Research I universities in the country. The Department of Accounting is AACSB-accredited with 500 undergraduate and 100 graduate accounting students and the College of Business Administration has 2,500 undergraduate and 1,000 graduate students. Please submit *curriculum vitae*, representative publications, and three letters of references to: Ram T. S. Ramakrishnan, CBA Accounting Dept, MC 006, 601 S. Morgan St., Chicago, IL 60607 or email: rramakri@uic.edu by February 24, 2006. UIC is an AA/EOE.

147 CHINESE UNIVERSITY OF HONG KONG, School of Accountancy invites applications for the post of Assistant Professor(s)/Associate Professor(s)/Professor(s) or Visiting Scholar(s) (Ref. 05/178(020)/2). Applicants should have a Ph.D. degree (those pursuing a doctoral degree close to completion may also apply). Please consult the full text of the advertisement at http://www.cuhk.edu.hk/personnel. Appointment(s) will be made on a fixed-term contract basis for one to three years. Visiting appointments of shorter duration may also be arranged. Applications will be accepted until filled. Please send full resume, copies of academic credentials, a publication list and/or abstracts of selected published papers, together with names, addresses, and fax numbers/email addresses of three referees to whom the applicants' consent has been given for their providing references (unless otherwise specified) to: Professor James Xie, Director of School of Accountancy, The Chinese University of Hong Kong, Shatin, Hong Kong or email: xie@baf.msmail.cuhk.edu.hk. Please quote the reference number on cover.

148 IOWA STATE UNIVERSITY, Department of Accounting invites applications for a tenure-track appointment to begin Fall 2006. Applications for Assistant, Associate, or Professor will be considered. Applicants should possess the commitment and demonstrable ability to be productive scholars and excellent teachers. All areas of accounting are encouraged to apply. Minimum requirements include an earned doctorate or expected completion of the doctoral degree prior to the beginning of the appointment. At the Associate Professor or Professor level, demonstrated achievement in scholarly research is expected. The teaching load will be designed to allow time for scholarly activities. Applications considered until position is filled, but should be sent by January 1, 2006, to: Dr. Richard B. Carter Chair, Iowa State University, Department of Accounting, 2330 Gerdin Business Building, Ames, IA 50011-1350 or via email to: rbcarter@iastate.edu. Submit a current *curriculum vitae*, a working paper, and names and addresses of three references.

149 TEXAS SOUTHERN UNIVERSITY, AACSB-accredited Jesse H. Jones School of Business invites applications for two (2) tenure-track positions at the Assistant, Associate, or Full Professor level beginning Fall 2006. Applications in all areas are welcome. A Ph.D. in Accounting (or ABD in advanced dissertation stage) is required. Professional certification and experience are preferred. A successful candidate will demonstrate a commitment to teaching excellence, scholarship, and service. TSU, the second largest HBCU in the nation, is located near the heart of downtown Houston, home to 18 *Fortune* 500 companies. Houston's robust business economy has a strong energy base as well as high-technology industries, medical research, and professional services. To apply, or for more information, contact: Dr. Cathy Claiborne, Texas Southern University, Jesse H. Jones School of Business, 3100 Cleburne St., Houston, TX 77004; Phone (713) 313-7771; Email: claibornemc@tsu.edu. Apply online at https://jobs.tsu.edu.

150 NORTHEASTERN UNIVERSITY anticipates openings for two tenure-track positions effective September 2006 at the Assistant or Associate Professor rank in Accounting. Candidates must possess the doctoral degree and have a strong commitment to excellence in teaching and scholarly research. Candidates with an established publication record, teaching experience in financial, managerial or auditing, and some business background are preferred. Salaries are competitive and negotiable. Faculty are actively encouraged to engage in research and publishing activities. Support is available in terms of summer funding, release time from teaching, graduate Assistants, and travel. Teaching is at the undergraduate and graduate levels, with graduate teaching in the M.B.A., M.S./M.B.A., M.S.A., and M.S. Tax programs. Send resumes to: Dr. Marjorie Platt, Accounting Group, 404 Hayden Hall, Northeastern University, Boston, MA 02115: Phone: (617) 373-4647: Email: m.platt@neu.edu. Northeastern University is an Affirmative Action/Equal Opportunity/ Title IX Employer. Minorities, women, or persons with disabilities are encouraged to apply.

151 NORTHEASTERN UNIVERSITY, College of Business Administration is seeking applications for the Joseph M. Golemme Research Professorship in Accounting. The Professorship is a tenured faculty position with a five-year renewable appointment. Applicants—at the Associate or full level—are expected to be actively engaged in research and publication and to provide research leadership. Additionally, applicants will be expected to teach in the Graduate School of Professional Accounting and in other degree programs in the College. Teaching will be at a reduced level. Candidates must have a doctoral degree and the highest level of accomplishment or promise in both scholarly research and graduate teaching. Please send applications with current resume to: Marjorie Platt, Accounting Group Coordinator, College of Business Administration, Northeastern University, 404 Hayden Hall, 360 Huntington Avenue, Boston, MA 02115; Email: m.platt@neu.edu. Northeastern University is an Equal Opportunity/Affirmative Action, Title IX University. Women and minorities are encouraged to apply.

152 STATE UNIVERSITY OF NEW YORK AT BUFFALO, School of Management invites applications for an accounting faculty position starting Fall 2006, subject to budgetary approval. This is a full time, non-tenure-track position with renewable contracts. Teaching load is four sections (12 credit hours) per semester along with an expectation of some departmental service. Research and publication is encouraged. M.B.A. or other applicable masters degree or Ph.D. required, CPA or other professional certification preferred. Candidates should have teaching experience in cost accounting, accounting systems or auditing, with tangible evidence of teaching excellence. Salary and fringe benefits are competitive. Send curriculum vitae, two letters of recommendation and teaching evaluations to Professor Ronald Huefner, Recruiting Chair, Department of Accounting and Law, Jacobs Management Center, State University of New York at Buffalo, Buffalo, NY 14260-4000; Fax: (716) 645-3823; Email: rhuefner@buffalo.edu. SUNYAB is an Affirmative Action/Equal Opportunity Employer.

153 STATE UNIVERSITY OF NEW YORK AT BUFFALO, School of Management invites applications for one or more expected accounting faculty openings starting Fall 2006, subject to budgetary approval. Rank is open, but we are especially interested in senior applicants in the auditing, financial, managerial, and systems areas. Applicants must demonstrate capacity to undertake scholarly research publishable in leading journals and to provide quality teaching at the undergraduate, M.B.A., and/or Ph.D. level. Doctorate in hand or (at the Assistant level) near completion from a reputable research university is expected. A substantial research record is a requirement for senior positions. Salary and fringe benefits are competitive. Send *curriculum vitae*, two letters of recommendation, dissertation proposal if appropriate, other research paper(s), and teaching evaluations to: Professor Ronald Huefner, Recruiting Chair, Department of Accounting and Law, Jacobs Management Center, State University of New York at Buffalo, Buffalo, NY 14260-4000; Fax: (716) 645-3823; Email: rhuefner@buffalo.edu. SUNYAB is an Affirmative Action/Equal Opportunity Employer.

154 DREXEL UNIVERSITY, LeBow College of Business, Department of Accounting and Taxation invites applications for the Clarkson Professorship in Accounting. Qualified candidates should be recognized scholars and have strong interest in supervising doctoral students and teaching at the doctoral level. The Chairholder will be expected to foster relationships between the Department and leaders in the Accounting Profession. Located in Philadelphia's University City area, Drexel's LeBow College of Business is AACSB-accredited at the graduate and undergraduate levels and offers doctorate, M.B.A., Executive M.B.A., and undergraduate degrees. LeBow has been recognized by *Financial Times*, *U.S. News & World Report*, and *Entrepreneur* for superior programs and the College is in a period of exponential growth in quality, achievement, and reputation. Send application materials to: David R. Campbell, Head, Department of Accounting and Taxation, LeBow College of Business, Drexel University, 3141 Chestnut Street, Philadelphia, PA 19104. Drexel University is an Affirmative Action/Equal Opportunity Employer.

155 UNIVERSITY OF HARTFORD, Barney School of Business, seeks a tenure-track Assistant Professor in Accounting, appointment beginning September 2006. Specialization in financial reporting is preferred. Professional certification and experience in teaching auditing in addition to financial reporting are desirable. Candidates must hold a doctorate in Accounting or anticipate completion by September 2007. Faculty members teach three courses per semester. Expectations include superior teaching, scholarly activities, institutional service, and interacting with the business community. The Barney School is accredited by AACSB-International and enrolls over 1,200 students in seven undergraduate majors and its M.B.A. and Master of Science in Accounting and Taxation programs. Apply to Dr. Paul Mihalek, Barney School of Business, University of Hartford, 200 Bloomfield Avenue, West Hartford, CT 06117-1599. Review will begin immediately and continue until the position is filled. Members of under-represented groups are encouraged to apply. The University of Hartford is an EEO/AA/M/F/D/V.

156 GRAND VALLEY STATE UNIVERSITY, Seidman College of Business invites applications for positions as Visiting Professors or Instructors in Accounting beginning Fall 2006. Need master's in accounting or related field, Ph.D. or D.B.A. preferred. Professional certification desirable. Financial Accounting, Cost/Managerial Accounting, and Tax are primary needs; other fields will also be considered. Applicant must demonstrate the ability to teach effectively. The Department and College are both accredited by the AACSB. Salary is competitive. For consideration, apply online at http://www.gvsujobs.org, attach resume, three references, sample of teaching evaluations and, if available, copies of publications or representative research. Positions will be open until filled. Grand Valley State University is an EO/AA Institution.

157 SYRACUSE UNIVERSITY, Lubin School of Accounting invites applications and nominations for the endowed Lubin Presidential Chair in Accounting at the rank of Associate or Full Professor. Candidates are expected to have an earned Ph.D. in accounting. Responsibilities include mentoring doctoral students and junior faculty, conducting research leading to publication in the top refereed journals in accounting, teaching at the undergraduate and graduate level, and academic and professional service. Applicants in all areas will be considered with a preference for those with an interest in financial accounting. Application materials should be sent to: Randy Elder, Director, Joseph I. Lubin School of Accounting, Martin J. Whitman School of Management, Syracuse University, 721 University Avenue, Syracuse, NY 13244-2450 or by email to rjelder@syr.edu. Review of applications begins immediately and continues until the position is filled. Syracuse University is an Affirmative Action/Equal Opportunity Education institution. Women and minority candidates are encouraged to apply.

158 STONEHILL COLLEGE Department of Business Administration has one tenure-track position in Accounting available at the Assistant/Associate level beginning September 2006. The ideal candidate will hold a Ph.D./D.B.A. in Accounting with strong interest in teaching and scholarship. Preferred areas of teaching include financial accounting, international accounting, and advanced accounting topics. Other areas include cost management, managerial accounting, accounting theory, and accounting information systems. ABDs have made substantial progress toward dissertation completion. Stonehill College is ranked #1 in the North by *U.S. News & World Report.* Salary/benefits are AACSB competitive. Teaching loads are nine hours per semester. For further information visit http://www.stonehill.edu. Candidates should submit an application letter, current *c.v.*, evidence of teaching effectiveness, current research papers/dissertation, and contact information for three references to: Russell Boisjoly, Business Department Chairperson, Stonehill College, 320 Washington Street, Easton, MA 02357; Email: rboisjoly@stonehill.edu. Stonehill College is an equal opportunity employer committed to diversity.

159 SAN FRANCISCO STATE UNIVERSITY has two tenure-track faculty openings in the areas of Financial Accounting and Managerial Accounting. The appointment date is Fall 2006. Candidates must have a Ph.D. or D.B.A. at time of employment, and have a strong commitment to both research and teaching. Professional experience or certification is desirable. The position will be filled at the rank of Assistant/Associate Professor. The Department of Accounting offers concentrations in Accounting for BS, M.B.A., and M.S.B.A. degrees. The College of Business is AACSB-accredited at both the undergraduate and graduate levels. Send application letter, *curriculum vitae*, teaching evaluations, and three letters of reference to: Dr. Scott I. Jerris, Chair, Accounting Hiring Committee, College of Business, San Francisco State University, 1600 Holloway Avenue, San Francisco, CA 94132. Applications accepted until the positions are filled. SFSU is an Affirmative Action/Equal Opportunity Employer.

160 THE UNIVERSITY OF AKRON, School of Accountancy invites applications for a tenure-track appointment at the Assistant or Associate Professor level beginning August 2006. Candidates' primary teaching and scholarship interests should be auditing. Ability to teach in a second area is a plus. Expectations include teaching undergraduate and graduate courses, conducting publishable research, and service. Preference will be given to candidates with professional certification, professional experience in auditing and accounting, and ability to work closely with the professional community. A Ph.D. is the minimum academic credential. Review of applications will be ongoing until the position is filled. Applicants should send a cover letter, names of three references, and *vita* to: Thomas Calderon, College of Business Administration, The University of Akron, Akron OH 44325-4802; Phone: (330) 972-6228; Email: tcalderon@uakron.edu.

161 CALIFORNIA STATE UNIVERSITY, SAN MARCOS invites applications for Assistant or Associate Professor, tenure-track positions beginning Fall 2006. Cost/Managerial and Financial candidates are preferred. Applicants must possess a doctorate in Accounting from an AACSB-accredited university by time of appointment, be committed to rigor in teaching and research, and willing to serve constituents in the University and business communities. The College of Business is rapidly growing with approximately 1,800 undergraduate and 140 M.B.A. students. CSUSM is located north of San Diego in a region that offers many unique benefits including an incredible climate. Applicants should send an application letter, *vitae*, three recommendation letters, and evidence of rigorous teaching and scholarship to: Accounting Search Committee, College of Business Administration, California State University, San Marcos, San Marcos, CA 92096-0001. Application review begins December 1, 2005. CSUSM is an Equal Opportunity/Title IX employer. For more information please visit http://www.csusm.edu/cba.

162 CALIFORNIA STATE UNIVERSITY, LOS ANGELES invites applications for full-time faculty positions in the areas of Financial, Cost/Managerial, Auditing, Forensic Accounting, Accounting Information Systems, Governmental, International, and Tax. Start date is September 1, 2006. Rank from Assistant to Full Professor and tenure status are dependent upon the appointee's qualifications and experience. A doctoral degree must be from an accredited institution of higher education in accounting, tax, or a closely related field. For candidates with a tax background, a doctorate is not required if the candidate has three years of full-time experience other than teaching, and either: (1) LL.M. in taxation and CPA or CMA certification; or (2) J.D. and a business or accounting master's degree with a concentration in taxation and passage of a State Bar Examination. ABDs will be considered. Ph.D. or equivalent is required for tenure. Preference will be given to candidates with Ph.D. or equivalent at the time of employment. Please send a letter of application, *curriculum vitae*, three letters of recommendation, and official transcript from institution awarding highest degree to: Ms. Susan Bergstrom, Assistant to the Dean, College of Business and Economics, CSULA, 5151 State University Drive, Los Angeles, CA 90032-8120. An Equal Opportunity/Title IX Employer. Please see detailed descriptions of positions at http://www.calstatela.edu/academic/position/csla_fac.htm.

163 FERRIS STATE UNIVERSITY is seeking to fill two positions: JOB CODES AR-2645 and AR-2600. Applicants from all areas of accounting are encouraged to apply; however, preference will be given to candidates with relevant teaching and business experience in the area of taxation (AR-2645) and accounting information systems (AR-2600). Teaching experience in the areas of financial accounting is a plus. Required: Doctorate in Accounting, or accounting certification in conjunction with doctorate in related field, or ABD with evident progression toward degree. Review of applications will begin immediately and continue until filled. Please submit cover letter, resume and names and phone numbers of three current references or three letters of recommendation, and copies of college transcripts to: JOB CODE [provide code of the position being applied for], Ferris State University, 420 Oak St., PRK-150, Big Rapids, MI 49307. For complete position postings, please visit http://www.ferris.edu/htmls/jobs/. An EO/AA Employer.

164 SOUTHERN CALIFORNIA EDISON is seeking a Senior Auditor. Location: Irvine, CA. Job Requirements: Bachelor's degree in Accounting, Business, or related experience; 10+ years experience performing financial and contract audits or related functions; demonstrated experience utilizing current FASB pronouncements, GAAP, and auditing standards; knowledge of the Sarbanes-Oxley Act; audit-related certification or license, such as CPA or CIA. Preferences: M.B.A./M.A./M.S. Familiar with power generation, energy trading, electric utility regulatory accounting and compliance, benefits plans, and corporate tax laws. Typical Responsibilities: Independently conducts, leads, directs, and/or participates as a team member on complex, sensitive, and/or specialized projects or audits. Assignment may include Sarbanes-Oxley Section 404 compliance testing, procurement contract audits, joint venture audits, benefit plan compliance, fuel and purchase power agreement audits, and other financial/accounting related audits. Requires domestic travel. If you are interested in this position and to learn more, please submit your resume in confidence by visiting http://www.edisonjobs.com. EOE.

165 PENNSYLVANIA STATE UNIVERSITY AT HARRISBURG is seeking an instructor of accounting and business law. The AACSB-accredited School of Business at Penn State Harrisburg invites applications for a one-year (with possible extension) full-time accounting instructor beginning Fall 2006. J.D. and CPA certification is preferred; J.D. with master's degree will be considered to teach undergraduate courses in business law, accounting principles, and taxation. Prior teaching and professional experience preferred. Reviews begin immediately; continuing until position is filled. Send résumé (or *curriculum vitae*) and four references to: Accounting Search Committee, c/o Mrs. Dorothy Guy, Penn State Capital College, 777 W. Harrisburg Pike, Box AAA, Middletown, PA 17057-4898. Penn State is committed to Affirmative Action/Equal Opportunity, and the diversity of its workforce.

166 CORNERSTONE RESEARCH, an economic consulting firm specializing in analyzing economic, financial, accounting, and marketing issues, is interested in hiring full-time and summer associates for its offices in California; Boston, MA; New York, NY; and Washington, D.C. Cornerstone Research provides an interesting and rewarding work environment. Associates work directly with esteemed faculty experts in a distinctive "partnership" that combines the strengths of the academic and business worlds. We seek Ph.D. candidates with the ability to apply academic research to real-world issues, present concise explanations of complex analyses and independently manage projects and junior staff. The ideal candidate will possess a strong empirical background in Financial or Cost Accounting, or Auditing, as well as demonstrate excellent interpersonal skills. We are also seeking experienced candidates for more senior levels. Interested candidates should send a cover letter that includes a statement describing your interest in economic consulting and location preference, a resume, a sample research paper and letters of reference to: Recruiting Assistant-Accounting, Cornerstone Research, 1000 El Camino Real, Suite 250, Menlo Park, CA 94025; Email: associate-recruiting@cornerstone.com; Website: http://www.cornerstone.com.

167 KENNESAW STATE UNIVERSITY, Coles College of Business, Department of Accounting invites applications for three tenure-track positions to begin Fall 2006. Applications for all ranks will be considered. Applicants should possess the ability to be productive scholars and excellent teachers. An earned doctorate from an AACSB-accredited university is required, and a CPA is preferred. The business and accounting programs within the Coles College of Business are fully accredited by AACSB-International. Applications considered until position is filled, but should be sent by December 31, 2005 to: Dr. Kathy S. Schwaig, Associate Professor, Department of Accounting, Kennesaw State University, 1000 Chastain Rd., Kennesaw, GA 30144-5591; or via email: kschwaig@kennesaw.edu. A complete application requires a letter addressing the candidate's qualifications for the position including evidence of effective teaching; current *curriculum vitae*; contact information for at least three references; and official graduate transcripts. Website: http://www.kennesaw.edu/facultypositions/

168 LONG ISLAND UNIVERSITY–C.W. POST CAMPUS, College of Management seeks applications for an accomplished Associate Dean who will also serve as Director of its School of Professional Accountancy. The successful candidate will hold an earned doctorate, be academically qualified, and have a demonstrated record of scholarly achievement and effective teaching. CPA licensure and administrative experience as an Accounting Department Chair/Director or Associate Dean at an AACSB-accredited institution are highly desirable. The individual should possess leadership skills that promote excellence in student learning; faculty, curriculum, and alumni development; and the expansion of the quality and quantity of the student body. A letter of application, references, and a curriculum vitae should be sent in writing or electronically to Dr. Charles A. Barragato, Long Island University–C.W. Post Campus, Brookville, NY 11548-1300; Email: Charles.Barragato@liu.edu. Long Island University is an Equal Opportunity Educator and Employer. Minority and women candidates are especially urged to reply.

香港浸會大學
HONG KONG BAPTIST UNIVERSITY

DEPARTMENT OF ACCOUNTANCY AND LAW

Associate Professor/Assistant Professor (PR036-2/05-06)

The appointee is expected to teach accounting subjects (in Financial Accounting, Management Accounting, or Accounting Information Systems), and supervise student projects at both the undergraduate and postgraduate levels.

Applicants should possess a relevant PhD degree. A record of good teaching at the tertiary level and strong research background are required. He/She is also expected to perform administrative duties in addition to teaching and research. A relevant professional qualification, knowledge of local accounting practices and professional connections will be an advantage, although not mandatory requirements. Initial appointment will be made on a fixed-term contract of two years commencing 1 September 2006. Re-appointment thereafter is subject to mutual agreement.

Rank and salary will be commensurate with qualifications and experience.

Application Procedure:

Applicants are invited to write in response to the requirements and provide an updated curriculum vita and/or fill in the application form which is obtainable by downloading from http://www.hkbu.edu.hk/~pers. The application should be sent to the Personnel Office, Hong Kong Baptist University, Kowloon Tong, Hong Kong. Applicants are requested to send in their most recent teaching evaluation results and samples of three publications. Applicants should also request two referees to send in confidential letters of reference to the Personnel Office direct. Please quote PR number on all correspondence. Details of the University's Personal Information Collection Statement can be found at [http://www.hkbu.edu.hk/~pers/job]. The University reserves the right not to make an appointment for the posts advertised, and the appointment will be made according to the terms & conditions then applicable at the time of offer.

Closing date: 15 March 2006

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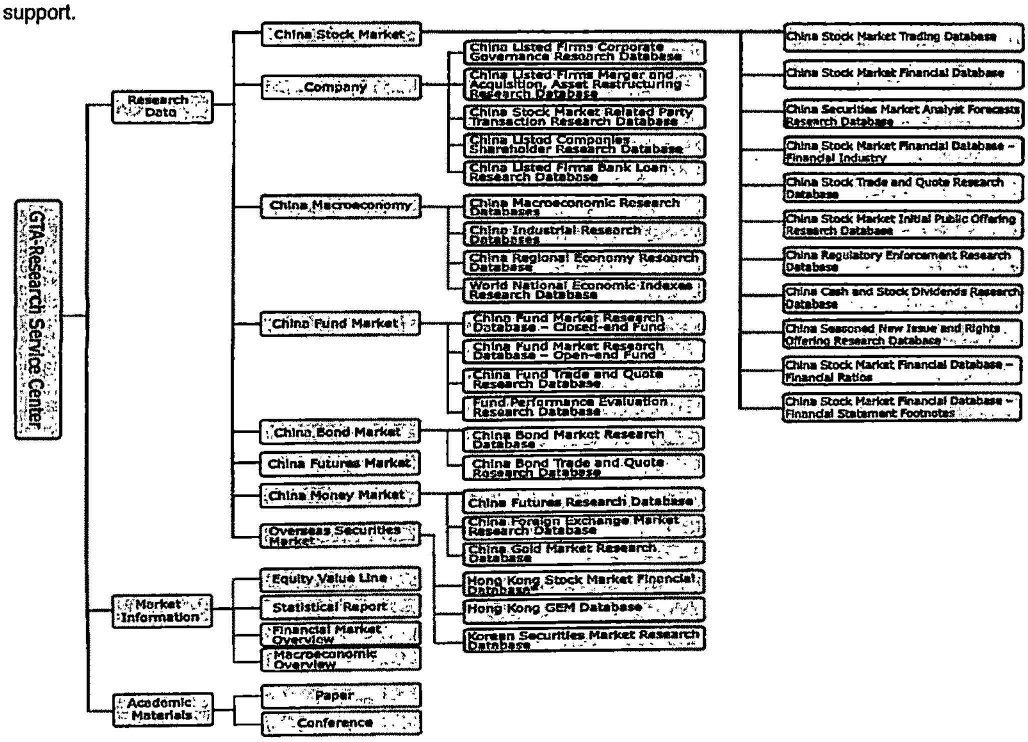
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THE ACCOUNTING REVIEW
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